OBITUARY NOTICE

SIR CHARLES SCOTT SHERRINGTON, O.M., G.B.E., F.R.S., 1857–1952

C. S. Sherrington, who died on 4 March 1952 at the great age of 94, was regarded by his contemporaries as an outstanding figure in the history of physiology. By his skill in devising and conducting experiments, by his cautious but penetrating interpretations of their results, and by his imaginative insight he revolutionized views on the working of the central nervous system and built a firm foundation on which others could confidently add to knowledge. Unless one turns to the text-books of sixty or seventy years ago it is difficult to realize how great the revolution has been. That muscular activity is as much concerned with the maintenance of posture as with the execution of movements; that 30–50% of the fibres in a nerve trunk distributed only to skeletal muscle are afferent; that in movements, whether reflex or voluntary or excited by electrical stimulation of the motor cortex, antagonistic muscles are reciprocally innervated; that many complex movements can be analysed into combinations of simple reflexes in which central excitation and central inhibition are algebraically summated; that muscular contraction is modified and regulated by impulses entering the central nervous system from proprioceptive endings in the contracting muscles themselves; that decerebrate rigidity (which he described in 1898) is the basis of reflex standing, albeit a caricature of that posture: these are among the fundamental principles which he had established prior to the publication of *The Integrative Action of the Nervous System* in 1906. During a further thirty years of active experimental work his attention was fruitfully devoted to spinal rhythmical reflexes, to the motor cortex of anthropoid apes, to the stretch reflex (with E. G. T. Liddell) and other factors in the maintenance of posture, to the role of central inhibition in the co-ordination of movements, and to further analysis of the crossed extension reflex and (particularly with J. C. Eccles) of the spinal ipsilateral flexion reflex.

This, however, is not the place to attempt an evaluation of his contributions to experimental physiology, which indeed were so great that it is impossible at present to see them in true perspective. Nor do I intend to recount the outward events of his life or the honours which were bestowed on him. Rather do I wish to convey some impression of the man whom I knew for the last thirty-five years of his life, and to say something about his outlook, his methods of work and of expression, and a few publications which are likely to be of particular interest to psychologists.

From its beginning in 1904 he served on the editorial advisory panel of the *British Journal of Psychology*. To the first number he contributed a paper 'On binocular flicker and the correlation of activity of "corresponding" retinal points' which ranks with Wheatstone's first description of stereoscopic vision as one of the most illuminating experimental studies of the problems of binocular vision. The framing of the questions to which he sought answers, the ingenuity of the apparatus which he devised, and the cautious but compelling interpretations which he put upon his observations illustrate his clarity of thought and mastery of experimental technique. That paper finally disproved the view that single vision with two eyes is due to anatomical confluence at an apsychical level of the twin paths from corresponding points of the two retinas. On the contrary,

'each uniocular mechanism develops *independently*—at least as to steadiness of brightness and intensity of brightness—a sensual image of considerable completeness. The singleness of the binocular perception. . . is the product therefore of a psychical synthesis that works with already elaborated sensations contemporaneously proceeding'. 'It is much as though, of the left and right lantern images, each were seen by one of two observers, with similar vision, and as though the minds of the two observers were combined to a single mind.' The argument was further developed in the last chapter of *The Integrative Action* and again in the new preface to the 1947 edition of that book. ('The cerebral seats of right-eye and left-eye visual images are. . . separate. Conductive paths no doubt interconnect them, but are shown to be unnecessary for visual unification of the two images.')

The physiology of vision interested him throughout his life. Before he was thirty he had collected copies of most of the monographs on the subject and knew the literature well. He described important new observations on contrast phenomena and flicker, as well as on movements of the eye and the sensual role of proprioceptors in the extrinsic ocular muscles.

A very different problem which he tackled experimentally is the origin of emotions. Does the 'psychical part' of an emotion precede the expressive movements and visceral changes which normally accompany it, or was William James right in postulating that our feeling of the bodily changes as they occur is the emotion? Sherrington found that dogs in which the thoracic and abdominal viscera as well as the trunk and limbs were severed from connexion with the brain by section of the vagi and spinal transection still showed undiminished signs of affection, joy, anger, rage, disgust, fear and sexual attraction. He concluded that the theory that visceral sensations are necessary to emotion is untenable, and that it is likely 'that the visceral expression of emotion is *secondary* to the cerebral action occurring with the psychical state'. Although visceral and organic sensations and the memories and associations of them may contribute to primitive emotions, they reinforce rather than initiate, and count for relatively little.

After he had retired from his professorship and from experimental work in 1935, his intellectual activity continued with unabated zeal until the moment when death suddenly, without warning, overtook him. He read and wrote and enjoyed the company of friends. He had for long been interested in the relation between mind and brain. His presidential address to the British Association in 1922 touched on the problem and he returned to it in his Rede Lecture at Cambridge in 1933. In 1937 and 1938 he gave a series of Gifford Lectures at Edinburgh which were afterwards published with the title *Man on his Nature*. Outside physiological circles this book is probably the best known of his writings. It is remarkable both in its matter and in its style. It exemplifies the width of his interests in history, philosophy, the arts and science; the depth and clarity of his thought; and the humility and sense of wonder of his approach to fundamental problems. To the question how is the mind attached to the body he could find no satisfactory answer. He produced a revised edition of the book less than a year before his death. In connexion with it he also published in 1946 an erudite and well documented account of the sixteenth-century French physician Jean Fernel. Noteworthy, too, was the publication in 1942 of a lecture *Goethe on Nature and on Science* (revised edition 1949).

A word should be said at this point about his highly individual literary style. Most readers of this *Journal* will be aware that his writings commonly require close attention on the part of the reader. The thread of the argument is liable to be lost if a single sentence

neglected. He wrote and rewrote almost everything until he was satisfied that his meaning could not be misunderstood and that no unnecessary words were included. He often expressed regret that it was so much easier for an author to be ambiguous in English than in French, while of German scientific writers he regarded Aubert as a supreme stylist with his rigorous economy of words. It is the desire to be unambiguous and at the same time concise that accounts for the unusual construction of many of his sentences; it is never carelessness either of thought or expression. Lest a sentence torn out of its context should be misleading he inserted saving clauses which sometimes made its construction appear clumsy. To achieve the right emphasis in the fewest possible words he often arranged his words in an unusual order: 'Familiar to those who work with the decerebrate preparation must be the observation that passive flexion of the characteristically extended knee is felt to evoke some development of resistance of it against that passive movement.' It is rarely possible to rewrite a sentence in fewer words without some sacrifice of meaning. His style developed and matured throughout his life, reaching its culmination in *Man on his Nature*. Here are memorable passages of vivid description supporting a closely reasoned argument which is itself expressed in noble and imaginative language.

He introduced many new technical terms into neurology, sometimes new words, sometimes old words or combinations of words to which he gave special meanings. They were all intended to clarify argument or, more commonly, to economize words. Among those which have come into general use are synapse (and synaptic), perikaryon, proprioceptive, final common path, neurone pool, subliminal fringe, recruitment, fractionation, and occlusion. A few, such as myotatic (borrowed from Gowers) and motoneurone, have little advantage over less cumbrous words. To Sherrington has been attributed the first use of the term 'adequate stimulus' in the sense of natural as opposed to effective stimulus. This is not so; it had long been in use by German writers and Sherrington (1906) took it from Aubert's *Physiologie der Netzhaut* (1865) where adequate are distinguished from inadequate effective stimuli.

His experimental work is all characterized by extraordinary thoroughness and by caution in reaching conclusions. In his early years he had worked as a pathologist and microscopic anatomist. The histology of the central nervous system, of which his friend Ramon y Cajal was a master, was a lifelong interest and he regarded it as the essential basis of physiological work. In addition he had manual dexterity in surgical procedures and in the use of apparatus, and was ingenious in the design of new tools, such as the isometric mirror myograph, for the recording of results. He had an uncanny knowledge, partly born of long experience, of the sort of question to which nature would vouchsafe an answer, and a wonderful facility in devising the best means for obtaining that answer. When one considers the enormous volume and scope of his published work, the accuracy of his observations is no less remarkable than the logical soundness of what he deduced from them. In very few instances indeed have either been impugned by subsequent work.

He was a short man of spare build who showed no sign of fatigue at the end of an experiment requiring many hours of intense concentration in a warm darkened room. Myopia necessitated the use of glasses except for close work. In conversation he listened intently with his eyes fixed on the speaker. Every now and then his whole face would light up as he interposed some shrewd question or remark. As a teacher he was seen at his best in practical classes, particularly in the mammalian class which he introduced at Liverpool and continued at Oxford. He expected careful work and good instruments from his pupils,

with whom he would discuss on equal terms their experiments and controversial physiological questions of the day. Here his modesty, his quickness of mind, his encyclopaedic knowledge, his mastery of experimental technique, his cautious but almost infallible judgement, his receptiveness of new ideas, and his accurate memory of men and things shone to full advantage. As a lecturer he was less successful with students, though he took immense trouble in preparing his lectures. He hesitated and fumbled to find exactly the right words in his anxiety not to mislead. What is perhaps surprising in one so experienced is that the emotional strain of lecturing was always so great. Up to the date of his retirement it was notorious to those who had frequent occasion to consult him that it was unwise to approach him during the hour or more preceding and the half-hour following a lecture.

It would be difficult to exaggerate the intellectual stimulus that he provided to the many people of many nationalities who worked with him. His considerateness and his unassuming brilliance inspired intense loyalty. He had strong likes and dislikes but he very rarely spoke unkindly of the living or the dead. Jealousy and pettiness were completely lacking. He and Lady Sherrington (who died in 1933) were charming hosts and there was always animated conversation in their house on all sorts of topics. Both of them had travelled much and kept their eyes and ears open. Sherrington was an excellent linguist who read easily Latin, French, German, Italian, Spanish and Dutch. He had more than an amateur knowledge of French and Italian painting. He himself was a skilful draughtsman. He was particularly well read in French literature of the nineteenth century and in English poets of all ages. Of Keats he was an enthusiastic admirer. In 1925 he published a slim volume of verse *The Assaying of Brabantius* (second edition, enlarged, 1940). Some of the poems in this book are of great beauty and ingenious versification. Characteristically he described them in a private letter as 'some foot-prints of a lame follower after the Muse'.

In reference to the affairs of others he was among the discreetest of men. About some of his own he was also curiously reticent. One was his lifelong activities as a collector of books; his library was shown to very few. Another was his age which he kept a closely guarded secret until after his ninetieth birthday. It was probably a tactful move on the part of some of his friends when, on what several of them knew to be his eighty-first birthday, they sent him congratulations on reaching eighty. Only in the last few years did he care to speak about his age. 'I am glad I have lived for so long, for I have seen how so much has worked out.' His intellectual powers, his memory, and his interest in things were fortunately unimpaired to the end, crippled though he was latterly by rheumatoid arthritis and slight deafness. He was much beloved by all who knew him and is universally mourned both as man and as scientist.

R. S. CREED

A STUDY OF OUTPUT IN TWO SIMILAR FACTORIES

By S. WYATT

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I. Nature and scope of the inquiry

This article gives an account of the amount and probable causes of differences in output observed in the manufacture of prefabricated concrete houses in two separate factories under the same general management. The factories were similar in size, equipment and layout, yet for about a year the rate of production in one factory (A) was about 70% higher than in the other (B). The difference first became noticeable shortly after the introduction of a group bonus system in the two factories. This system involved (*a*) the timing of different component operations connected with the making of concrete slabs and posts, (*b*) an assessment of the amount of effort expended by workers in making these components, and (*c*) an allowance for relaxation. These timings, assessments and allowances were then used to compute the standard time per house, and if the house was completed in less than the standard time the workers received a bonus proportionate to the time saved. If, for example, the standard was 213·4 man-hours per house and the group completed a house in 200·2 man-hours, the time saved would be 13·2 man-hours and the bonus 13·2/200·2 of the basic rate.

The group bonus system was in operation in these factories from November 1946 to December 1948, and the investigation was carried out in 1948–9. The procedure included (*a*) the collection and analysis of factory records, and (*b*) systematic interviews with a 25% random sample of all production workers. Informal interviews were also held with workers' representatives, supervisors, managers, labour officers, medical staff and other officials.

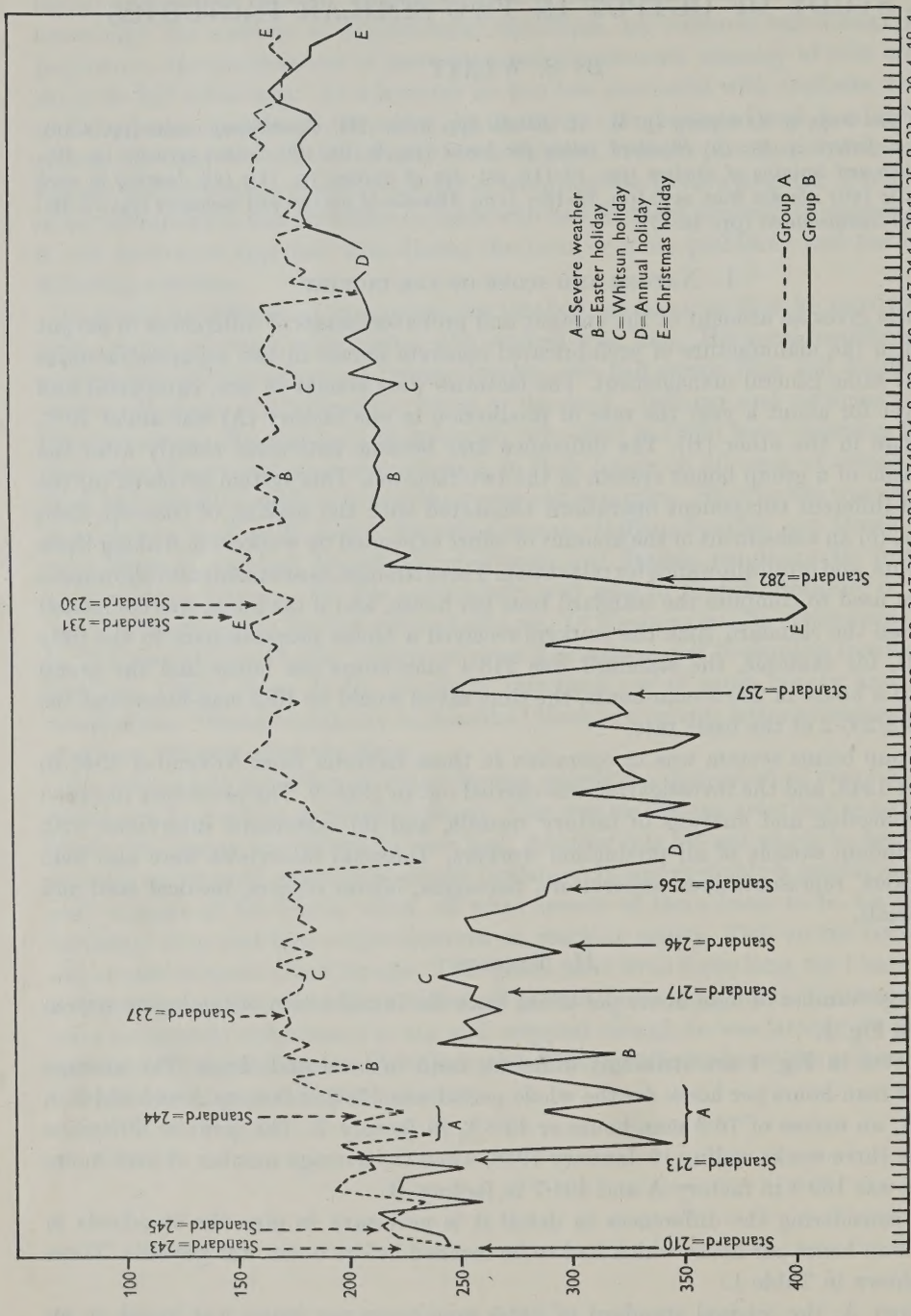
II. Results

The average number of man-hours per house since the introduction of the bonus system is given in Fig. 1.

The curves in Fig. 1 are strikingly different, both in level and shape. The average number of man-hours per house for the whole period was 178·3 in factory A and 254·6 in factory B; an excess of 76·3 man-hours or 42·8% in factory B. The greatest difference was in the three weeks ending 10 January 1948, when the average number of man-hours per house was 169·3 in factory A and 401·7 in factory B.

Before considering the differences in detail it is necessary to give the standards in terms of man-hours per house which had to be reached before bonus was payable. These were as shown in Table 1.

In factory A, the original standard of 243·3 man-hours per house was based on an expected output of two houses per day and was regarded as temporary as it was introduced before the time studies were completed. The subsequent changes in standards were due


A=Severe weather
B=Easter holiday
C=Whitsun holiday
D=Annual holiday
E=Christmas holiday
Group A
Group B
Standard=210
Standard=213
Standard=217
Standard=246
Standard=256
Standard=257
Standard=282
Standard=237
Standard=244
Standard=230
Standard=231
Standard=243
Standard=245
100
150
200
250
300
350
400
1947
1948
Dec. Jan. Feb. Mar. Apr. May June July Aug. Sept. Oct. Nov. Dec. Jan. Feb. Mar. Apr. May June July Aug. Sept. Oct. Nov. Dec.

mainly to slight alterations in the utilization of the concrete used in making slabs and posts.

In factory B the original standard of 195·8 man-hours per house was also based on an expected output of two houses per day, and the subsequent changes (listed in Table 1) show the varying levels of output which the men were expected to reach before any bonus could be earned. For approximately fourteen months the men refused to accept these standards and, largely as the result of representations from the trade unions concerned, each of the earlier standards was quickly followed by one or more concessions in the form

Table 1

Date of introduction	Standard (man-hours per house)	Notes
	Factory A	
25. xi. 46	243·3	Original temporary standard
16. xii. 46	244·8	Final standard for concrete slabs
24. ii. 47	243·5	Final standard for concrete posts
29. iv. 47	237·4	Concrete mixing values amended
29. xii. 47	231·1	Concrete mixing values amended
5. i. 48	229·6	Mixing and transport of concrete amended
	Factory B	
25. xi. 46	195·8	Original standard
25. xi. 46	210·1	Issued 10 xii. 46 but back-dated
27. i. 47	213·4	Standards for posts amended
12. v. 47	217·3	Standards for posts amended
9. vi. 47	246·1	Standards for posts amended
14. vii. 47	255·8	Change from iron to aluminium moulds
10. xi. 47	257·0	Work transferred to new building
22. i. 48	265·8	Standards for posts revised
22. i. 48	282·1	Standards for all components revised but back-dated to 22. i. 48 (issued 12. ii. 48)

of temporary allowances. The first of these was the addition of 25 % to the original standard and this was raised to approximately 60 % in February 1947 to compensate for the effects of unusually severe weather. As conditions improved the allowance was reduced and from March to September 1947, it tended to fall from about 40 to 25 %. The allowances in this period were due in part to time spent in improving concrete parts of doubtful quality or appearance and to some experimental work on aluminium moulds. In October and November, work was transferred to a new building and there were no standards. The operations were then retimed and new standards issued but the men continued to receive additional allowances. Even in 1948 when the men finally accepted the standard of 282·1 man-hours, and there were no material difficulties, they were granted an additional allowance of approximately 12½ %. Thus, throughout the two years of production the men were allowed more man-hours per house than the number fixed by the Production Study Department.

Throughout this inquiry it was repeatedly emphasized that the general conditions of work were approximately the same in factories A and B, and that the flow of production was temporarily interrupted in factory A as well as in factory B, yet the occasional allowances in factory A never exceeded 5 %. There can be little doubt that most of the additions to the basic standards in factory B were intended primarily to ease the official standards and to make them more acceptable to the men.

The bonus standards in factories A and B can now be considered in relation to the curves in Fig. 1. The curve for factory A shows a progressive but irregular improvement in the first four months after the introduction of the bonus standard followed by a fairly

steady rate of working which was usually less than 180 man-hours per house. The men were earning bonus almost from the start, and the amount increased as output increased. From April 1947, output exceeded the standard by approximately 36%. The slower rate of working in February 1947 was due to unusually severe weather which affected attendance and caused material shortages; the fall in August 1947 and in July 1948 was due to local holidays and their after-effects. In this factory there was no closed period, and holidays were 'staggered', usually throughout July and August.

The curve for factory B was, at first, not unlike that for factory A, but in January 1947 the men in factory B decided that the bonus standard, even with the temporary allowance of 25%, was too high and the curves began to diverge. An easing of the standard to 213·4 man-hours per house caused a temporary improvement, but early in February there was a sharp drop in the rate of working due to a severe blizzard which blocked the roads and dislocated transport. This set-back seemed to make the men more dissatisfied, and although better weather brought a slight improvement the rate of working was still much below the earlier level. By August 1947 it had slowed down to approximately 350 man-hours per house, although in the meantime the standard had been eased to 246·1 man-hours per house. At this stage the men were thoroughly unsettled and dissatisfied, and there was practically no change in attitude or output until November 1947 when production was transferred to a new building and the operations were re-timed. While the new standard was being considered by the trade union, the men agreed to 'have a go' and for two weeks they worked at an average rate of 252 man-hours per house. They soon decided, however, that the standard was 'not good enough' and within a few weeks their rate of working had slowed down to 400 man-hours per house. This alarming figure resulted in yet another easing of the standard (from 257·0 to 265·8 man-hours per house) in January 1948, but the men still objected and within a fortnight it was relaxed to 282·1 man-hours. The effect of this last concession was immediate and spectacular and the time taken to complete a house was almost halved. Output now seemed to be stabilized at a fairly satisfactory level, but towards the end of August 1948 there was another definite improvement. At this stage the men were told that the production of prefabricated concrete houses would cease at the end of the year and, in addition to their own programme of work, they were given the chance to take over the incompleted contracts of other firms, equivalent to 200 additional houses. An assurance was also given that, no matter how high their earnings, the standard rating for bonus would not be changed. The men decided to 'go all out while the going was good' and they began to work at an average rate of approximately 182 man-hours per house. They knew that, when production stopped, they would be transferred to other work where there was no chance of earning bonus for several weeks.

On the whole, the curves for group B reflect the attitude of the men to the initial and subsequent standard ratings. The immediate and ostensible cause of their low and irregular output in 1947 was the belief that the standard rating was too high. The management, on the other hand, claimed that the standard was fair and well within their reach. It is accordingly necessary to consider some of the possible reasons for this conflict of opinion and to explain why the output of factory B was so much lower than that of factory A.

III. Contributory causes

Broadly speaking, the observed differences in output in factories A and B could be due to material conditions or to human factors or to some combination of the two. It is seldom easy to discover the causes of events long after their occurrence, and it is still more difficult

to estimate their exact effects. However, a close scrutiny of the factory records and a fairly exhaustive questioning of people concerned with production brought to light the factors enumerated and discussed below.

(i) *Material factors*

The most important material factors were layout, equipment and method of manufacture. The production study engineers, who were familiar with the conditions in both factories, stated quite definitely that although the methods of production in certain operations were not strictly identical, the differences were slight and the effects on output negligible. This view was confirmed by the management and by a detailed study of the work specifications and time-study records. In any case, minor differences in method would have only a slight effect on the level of the output curves and not on their general trend. Thus it can be asserted with confidence that the widely different rates of production in groups A and B (Fig. 1) were not due to material factors but to other causes.

(ii) *The standard rating for bonus*

The production of prefabricated houses began in a small way in August 1946 in factory A and in July 1946 in factory B, and bonus standards were introduced in both factories in the last week of November 1946. Since the time and method of introduction of the bonus standards seemed to have some effect on the subsequent rate of working it is necessary to examine them in some detail.

In the first place, the number of men employed at the time of the introduction of the bonus standard was 153 in group A and 115 in group B. In the next six months the numbers increased by 21 in group A and by 80 in group B. Thus, when the bonus standards were introduced, both groups contained a number of men who were not fully experienced and more were added as work proceeded. This means that the bonus standards were applied when the groups were being built up and when the rate of working of many men was still improving. Since the building up process was more marked and continued for a longer period in factory B, the adverse effect on group output and earnings would be proportionately greater in factory B than in factory A.

Accurate figures of the rate of working before the introduction of the bonus standard were not available but they were certainly not less than 550 man-hours per house in factory A and 600 in factory B. Thus, in order to earn bonus, the men in factory A had to speed up their rate of working from approximately 550 to 243 man-hours per house and in factory B from at least 600 to 196 man-hours per house—a rather forbidding prospect. If the introduction of the bonus standard had been postponed for two or three months it is highly probable that, as the men became more experienced and proficient, the difference between actual and expected output would have been much less. In other words, a bonus standard which seemed out of reach in November might have been accepted as reasonable in the following January or February. Incidentally, it might be thought that certain difficulties would arise when attempting to fix a standard for men of varying experience but the production study engineers claimed that such doubts were groundless.

Secondly, the production study departments were informed by the general management on 15 November 1946 that the bonus scheme had been approved and must be introduced by 25 November. Although it was emphasized that the standards would be temporary and subject to amendment as conditions became more settled, it was futile to expect the

production study engineers to time the numerous operations, fix the standards, arrange the teams and explain the methods of work within a week. People familiar with time-study procedure can readily imagine the panic and rush caused by these instructions. Yet the work was done after a fashion and standards were introduced on 25 November. It so happened that the men in factory A accepted the standard but the production engineers in this factory admitted that the short time allowed greatly increased the possibility of error and that what happened in factory B might easily have happened in factory A.

There is, accordingly, reason to believe that, in both factories, and especially in factory B, the bonus standard was introduced too hurriedly and too soon. In factory A, the effects were mitigated to some extent by the introduction of a temporary standard which was amended as conditions became more stable and the time studies more precise. In factory B, the initial standard was regarded as fixed and final; it was feared that a temporary standard might be troublesome, especially if it had to be 'tightened' later. There was also some evidence that, in factory B, the standards were issued in a rather perfunctory manner but, in factory A, they were explained with some care and thoroughness.

(iii) *Labour turnover*

The composition of a working group seldom remains unchanged for any length of time, and the rate of change or labour turnover is often a measure of some importance. Reference has already been made to labour changes at the beginning of the bonus period but it is also necessary to consider the extent and probable effects of subsequent changes.

The figures of labour turnover collected in the present inquiry show the number of men who joined and who left the two groups of workers in 1947 and 1948.[1] They were:

	Group A			Group B		
		Number			Number	
Year	Average strength	In	Out	Average strength	In	Out
1947	154	113	129	171	171	87
1948	133	122	162	184	44	89

The intake of new workers was very noticeable in the first few months when the groups were being built up and it was greater in group B than in group A. Also, the weeding out of the less efficient workers was rather more drastic in group A than in group B. Thus, on the whole, the men in group A were more settled, experienced and suitable than those in group B, and hence were more likely to reach the bonus standard.

(iv) *Selection and training of workers*

In these two factories, workers were medically graded as A, B and C according to their degree of fitness for work. In the factory A group there were 90% in grade A, 7% in grade B and 3% in grade C. This group included many ex-Service men (56%) from the last war and the remainder were chosen as far as possible from the fittest men in other departments, many of whom volunteered for work on concrete houses. Further, most of the men in this group had spent two weeks in the training department where they were graded for aptitude and only those who satisfied the instructor were allowed to go on

[1] Most of the men (about 90%) who joined or left the groups were transferred from or to other departments of the firm.

production. The few men who received no preliminary training were supervised and helped by a demonstrator during their first week on production. Thus most of the men in group A were first selected for health and fitness and afterwards for aptitude. They were accordingly a highly selected group.

In factory B, there was a local rule that when workers were transferred from one department to another the 'last in should be the first out'. Thus the men in group B were drawn from the last entries to other departments and their suitability for work on concrete houses was accidental rather than designed. They were part of a 'floating' population and many were ex-miners. Very few had been medically graded but the management said that not more than 10% were in grade A and several were in grade C. Further, factory B had no training department; hence the men were neither selected nor trained for work on concrete houses.

The relatively low standard of labour in factory B was due partly to the effect of the trade depression in the 1930's when many of the fittest men left the district to seek employment elsewhere. Then, at the outbreak of war in 1939, most of the younger and fitter men who still remained joined the Forces so that when the factory opened in August 1940, the field for recruitment was limited and much of the labour was sub-standard. The number of men employed in factory B dropped from 4990 in 1945 (the peak period) to 1853 in 1948 and, because of the rule 'last in, first out' most of those now employed were recruited from the labour available in 1940. The proportion of ex-Service men in group B (12·0%) was much smaller than in group A.

Summarizing, it may be said that the men in group A were recruited from fitter workers and were more highly selected and better trained than those in group B.

(v) *Age of workers*

The age distribution of the men employed on the production of concrete houses showed that, in January 1947, the men in group B were, on the average, $6\frac{1}{2}$ years older than those in group A. In group B, 52·5% of the men were in the age groups 50–69 and 16·7% in the age groups 60–69; the corresponding figures for group A were 28·8 and 4·1%. Thus the average age and the proportion of older men were much higher in group B than in group A. If, in the higher age groups, working capacity decreases with increasing age, then group B would be less likely to reach the same level of proficiency as group A. Alternatively, if group B did reach this level, it would be at the cost of greater strain and fatigue. Actually, group B had to produce approximately 30% more than group A before they could earn bonus.

The age distributions in 1948 were fairly similar to those for January 1947 and therefore the age handicap in group B persisted throughout the bonus period. That the men in group B were aware of this disability is shown by the following verbatim comments:

'The timing's all wrong: the men are of different ages and the timing's too hard for some of them. Some jobs are too hard and some men have not the capacity to keep up.'

'A lot of the men are getting very old and they can't keep the pace. It's too much for them and they stop off. Time study is no good for old men and it's not fair to them.'

There were many variations on this theme from the men in group B but very few from those in group A.

(vi) *Journey to work*

The workers in both factories lived in towns and villages scattered over a wide area and the journey to work, usually by bus or train, was often long and sometimes difficult, especially in winter. Interviews with a 25% random sample of the men employed in groups A and B showed that 31% of the men in group A and 64% in group B took an hour or more to get to work. In group A, no man took more than $1\frac{1}{2}$ hours, but in group B, 20% needed from $1\frac{1}{2}$ to $2\frac{1}{2}$ hours to get to work. Some of the men in group B lived in remote and isolated hilly districts and a walk of one or two miles to get the bus was not uncommon. Further, the hours of work in factory B were 7.0 a.m. to 4.20 p.m. (4.15 on Friday), but in factory A they were 20 minutes later. The earlier start in factory B when combined with a longer journey to work made it necessary for some men to get up between 3.30 and 5.0 a.m. It accordingly seems probable that, on the average, the journey to work was more tiring for the men in group B and they would be less fit for work when they arrived. Incidentally, most of the workers in both factories travelled by special train or bus and if they overslept they were often 'out' for the day.

When the men were asked what they thought about working conditions, 45% of the sample from factory B referred to difficulties arising from the long and tedious journey to work. The corresponding figure for group A was only 8%.

(vii) *Absence from work*

Although absence from work is often due to causes outside the factory, it is usually a useful index of the effects of working conditions. It accordingly seemed desirable to compare the rates of absence of the men in groups A and B, and the combined figures for 1947 and 1948 are given below:

		Number of shifts lost									
	No. of shifts possible	Sickness		Injury		Leave		Without permission		Total	
Factory		No.	%	No.	%	No.	%	No.	%	No.	%
A	71,114	2894	4·07	238	0·33	220	0·31	1909	2·68	5261	7·40
B	86,920	1985	2·28	143	0·16	267	0·31	5938	6·83	8333	9·59

The figures of total absence were higher in factory B than in factory A. These figures can be accepted as correct since a worker is either present or absent and this information is easily obtained from the clock-cards. Difficulties arise in the classification of absences, especially those due to sickness and without permission. Thus in group A, sickness absence was higher and absence without permission was lower than in group B. There is not the least doubt that many of the absences without permission in group B were due to sickness and should have been recorded as sickness absence. That they were not so recorded was due mainly to the indifference of the people responsible for the enforcement of the rule relating to medical certificates.

In these factories, a worker absent through sickness is supposed to send a medical certificate after the third day of absence, but in factory B he could be absent for a week and sometimes longer, before he was asked to do so. In factory A, on the other hand, the sickness absence rate was known to be reasonably accurate and it was not unlike the rates recorded in other factories where the production of medical certificates was strictly enforced. On the whole, the classification of absences in factory B was a waste of time and effort, and any value it may have depends on the rather doubtful assumption that the degree of error remained fairly constant throughout the period.

From 4 September 1948, workers in these two factories with more than twenty-six weeks' service received the full basic rate of payment for sickness absence. They were, however, not paid for the first three days of absence unless the 'incapacity lasted at least five working days'. Since payment depended on the production of a medical certificate after the third day of absence there was now a strong inducement for workers to get them without delay. At the same time, the rule that a worker must be absent at least five days before he received payment for the first three days was a temptation to prolong short sickness absence.

The weekly absence before and after the introduction of sick pay is shown in the following curves (Fig. 2). For purposes of comparison, the absence rates for all men in factory B are also given.

The curves for factory A (concrete houses) and factory B (all men) show a substantial increase in total absence from the beginning of September, when sick pay was introduced, to the end of the year. Part of this increase may doubtless have been due to seasonal factors which usually cause a rise in sickness absence from August to December. As an indication of this seasonal trend, the following monthly absence records were obtained from a large group of factories scattered throughout the country:

Month	No. of men	Percentage absent	
		Sickness	All causes
June	62,685	2·88	4·73
July	60,898	2·77	4·72
August	61,102	2·84	4·89
September	61,247	3·07	5·01
October	61,062	3·26	5·23
November	60,877	3·47	5·38
December	60,784	3·52	5·89

Similar trends were found in other factories, so that there seems to be good ground for thinking that most of the much greater increase in sickness absence recorded in factory A (concrete houses) and factory B (all men) was due to the effect of sick leave with pay. In other words, minor ailments which were formerly ignored were now used as reasons for absence, and the workers seemed to have no difficulty in getting these reasons medically certified. It is equally probable that some workers invented ailments for this purpose. Incidentally, the converging tendency of the curves for factory B (all men) from September onwards supports the view previously expressed about the laxity in demanding medical certificates. When these were needed for sick pay they were automatically produced so that the proportion of sickness absence to total absence progressively increased.

The curves for factory B (concrete houses), unlike the other curves in Fig. 2, fail to show the striking increase in sickness absence after the introduction of sick pay although the men in this group were, on the average, older and less fit than those in factory A (concrete houses). It will be recalled that, about the beginning of September, the men in group B were told that the production of concrete houses would end in the following January and they were given the chance to complete 200 additional houses. This news caused a sharp increase in their rate of working and in their bonus earnings (£2. 5*s*. 0*d*. to £3. 15*s*. 0*d*. per man per week). The men knew that, when production stopped, they would be transferred to other work where, for several weeks, they would earn no bonus. There was accordingly an unusually strong incentive to earn as much as possible and any tendency to take advantage of sick leave with pay was more than neutralized by the lure of high bonus

earnings. During this period, minor ailments were ignored and some men came to work when they should have been at home. Typical comments were:

'We are now getting a good bonus but I doubt whether the men can keep this pace going. It's too much for some of them but they want the money.'

'Some of the men are not fit to be at work but they know what's in store for them and they're taking the chance while they can.'

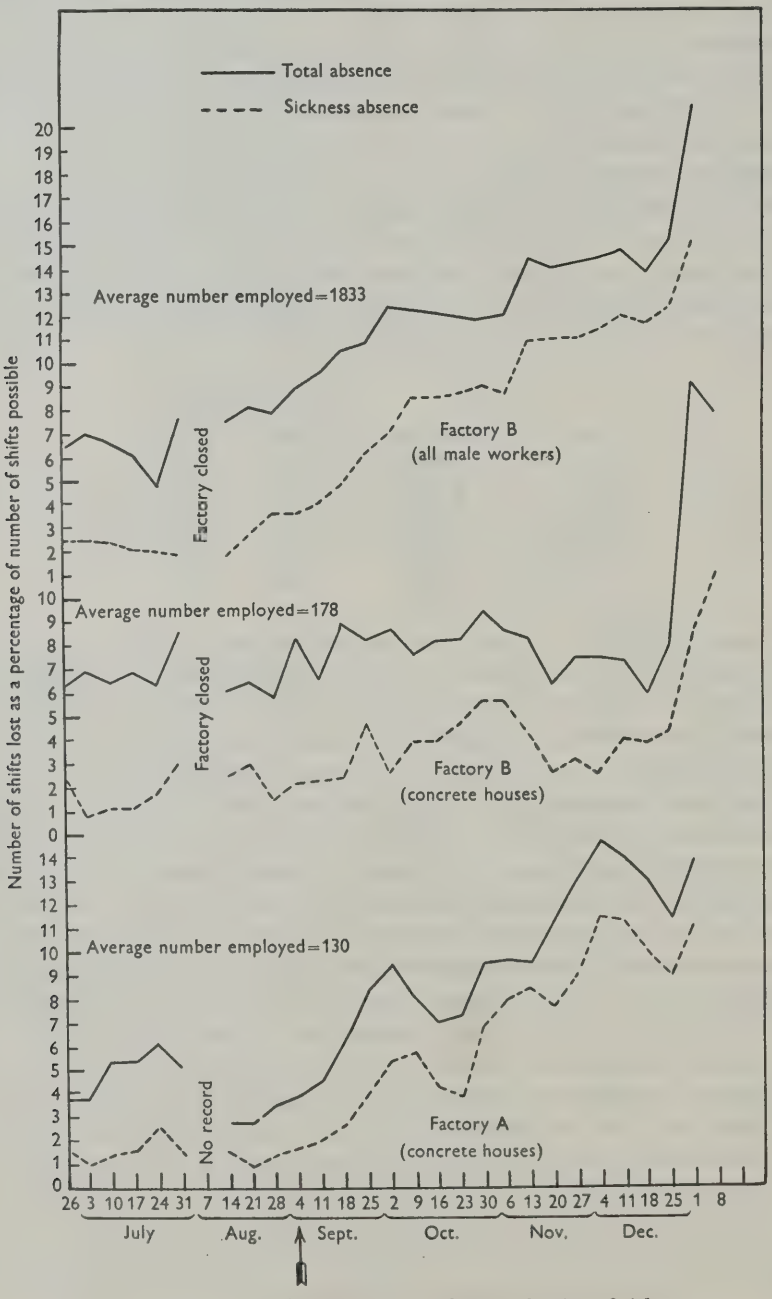


Fig. 2. Weekly absence before and after the introduction of sick pay.

In this factory, the production of concrete houses ended in the first week of January 1949, and it was no surprise to find a sharp increase in sickness absence among these men after their transfer to other work. There were signs that this was beginning to happen in the last two weeks when work was 'fading out' (Fig. 2), and in the next four months it fluctuated between 12·4 and 16·4%.

The men employed on concrete houses in group A were not told of the approaching end of production until early in November 1948 and, unlike group B, they were limited to the existing work schedule so that there was nothing to be gained by trying to do more in the time available. Further, in the latter part of 1948, many of the regular workers in group A were moved to a new department (concrete sleepers) and were replaced by trainees who were hardly likely to have much enthusiasm for a job that was drawing to its close. Finally, at the time when sick pay was introduced the bonus earnings of group A were only about half those of group B and they therefore had much less to lose if absent through sickness. Thus group A, as compared with group B, were less reluctant to give way to minor ailments and their rate of sickness absence increased.

(viii) *Attitude of workers and managers*

Many of the workers and all their shop stewards were interviewed in order to get their opinions on various factors associated with the conditions of work. As might be expected, there was a fairly widespread belief among the men in group B that the original bonus standard was much too high, and they went on to describe how they lost interest and became discouraged when they could not reach the standard. At the same time it was clear that many of the men did not object to the bonus system as such but to the way it was applied. Thus a fairly typical statement was 'I'm not against the system but there's no point in it when you can't reach the standard. If we could have earned a few shillings bonus at the start it would have helped a lot.'

It was also apparent that most of the men in group B had little confidence in the production study department. This distrust was confirmed by the production study engineers who pointed out that the workers were just as suspicious as they had been seven years ago when time study was first introduced. Some of this distrust may have been irrational but, whatever its cause, it induced a highly critical and unresponsive attitude to time-study methods and standards. In particular, the workers not only felt that the standards were too high but also that they were fixed without due regard to individual needs and capacities.

The probable causes of the low and irregular rate of working of the men in group B were discussed with managers and supervisors from assistant foremen upwards. They pointed out that most of the men were either ex-miners or came from mining families and that their attitude to work and to the management was largely due to past experience of hard times in a depressed area. It was also said that long experience of collective bargaining led them to expect an offer to be changed to their advantage and some of the managers regretted it could not be applied to standards based on time study which, 'because of their scientific nature admitted of no argument'.

Although most of the managers were inclined to blame the men and their leaders for the low output there were some who thought that 'the trouble began with the series of vague allowances' which were granted in the first place to compensate for a temporary shortage of equipment and were then continued 'for all kinds of reasons but mainly to keep the men at work'.

During the course of this survey, the higher management in factory B repeatedly asserted that the original bonus standard was accurate and just. As proof of this assertion they pointed to the high rate of working of 185 man-hours per house reached in the last few months of 1948. Since the original standard was 195·8 man-hours per house there seemed to be some justification for this assertion. It must be remembered, however, that at the end of August 1948 the men were told of the approaching end of the contract and of the possibility of producing an additional 200 houses before the department closed down. There was accordingly an unusually strong incentive to earn as much as possible while the opportunity lasted, and the management admitted that the men were doing as much as they could and in some cases more than they ought. It was certainly doubtful whether this final spurt could be sustained without severe strain, and it seemed unjustifiable to assume that the same rate of working was possible at the beginning of the bonus period when the conditions of work were unsettled and many of the men comparatively inexperienced.

IV. General considerations

The results of this survey show that the differences in output in groups A and B were due primarily to human factors and not to material differences in the type or conditions of work. The immediate cause of the differences was the introduction of a bonus standard which was accepted by group A and rejected by group B. In both factories, the bonus standard was introduced before the conditions of work and the rate of working had become stabilized, and the instability was greater in factory B than in factory A. Further, the original standard in factory A was regarded as temporary and subject to adjustment, but in factory B it was issued as permanent. Finally, the standard in factory B was much 'tighter' than in factory A. The introduction of the bonus standards in these two factories prompt the following comments:

(*a*) It is difficult, if not impossible, to fix a bonus standard before the methods and conditions of work are stabilized and the workers are fully experienced.

(*b*) If a standard is introduced while conditions are changing it should be issued as a temporary measure.

(*c*) The validity of a bonus standard depends in part on (i) the number and distribution of the timings made on each worker, and (ii) the number of workers timed. The time-study engineers were instructed to complete their studies within a week and their procedure was, in consequence, both hurried and superficial.

(*d*) The standards fixed by different time-study engineers are supposed to be largely independent of the engineer who fixes them. The results obtained in this survey do not support this claim.

The cause of the trouble in factory B was the belief that the bonus standard set by the time-study engineers was much too high. As already mentioned, the rate of working when the standard was introduced was lower in factory B than in factory A. Thus the difference between existing output and the bonus level was greater in factory B than in factory A and in the latter factory it was already unusually large. In such cases it is often desirable to reduce the gap between actual and expected output by means of intermediate bonus levels. Bonus earnings can start at any point, and the subsequent relation between earnings and output can take almost any form.

The feeling among the men in factory B that the bonus standard was too high could be explained in part by their reduced capacity for work. As already mentioned, 52·5% of these men were over 50 and 16·7% were over 60 years of age. Further, at least 90%

of the total were said to be unfit for heavy or strenuous work. Complaints of fatigue and strain were fairly common, and the evidence from this and other studies suggests that time-study engineers often overlook the aged or infirm when fixing their standards.

It should be mentioned that most of the men in factory B were not opposed to the bonus system of payment but to the way it was applied. They realized that, given proper and reasonable standards, the system provided a strong incentive to work and the bonus was a satisfying reward for effort expended. They deeply distrusted the production study department and this distrust was intensified by their inability to understand the procedure. They were overawed by the 'scientific' method and terminology, and felt helpless when the engineers talked of measured time, assessment of effort and allowances for fatigue and relaxation. For these and other reasons, it is important that time studies be made and standards fixed in the presence and with the aid of one of the workers' representatives who has been trained for this purpose. There is also much to be said in favour of explaining the principles and procedure in simple language to all the workers' representatives.

It is also relevant to mention that, as far as could be ascertained, not enough attention was given to motion study as a prelude to time study. Even a casual observer could not fail to notice several examples of crude and wasteful movements and clumsy arrangements of material both in the manufacturing of concrete posts and slabs and in other departments. Sometimes the operatives were aware of these sources of inefficiency but they kept to the old methods because, as one man said, 'if we suggest or make an improvement they only cut the rate and we are no better off'. Time study should always be preceded by motion study based on insight and common sense and some knowledge of psychological and physiological principles. Elaborate instruments may sometimes be useful but often they are merely impressive and costly refinements. When the best arrangement and methods of work have been determined, the operatives should be fully trained in these methods before any time studies are made and targets fixed. Time-study standards and targets based on wasteful movements are unsound in principle and uneconomical in practice.

From the evidence given in this report, the conclusion seems inescapable that the men in group B could not be expected to reach and maintain the same rate of working as those in group A and there was therefore some justification for their refusal to accept the series of bonus standards issued by the production study department in the first seven months of the period. Also, if the material conditions of work in factory B made the original standard unworkable it should not have been introduced; alternatively, if material conditions had been normal the standard would have been too tight. Although the continued resistance of the men to the series of bonus standards was doubtless encouraged and prolonged by some of the shop stewards, it was largely a reaction to a deteriorating situation which, had the management shown more insight and understanding, would probably never have arisen.

During the last few years much publicity has been given to the effects of incentive payment schemes on output but little has been heard of their effects on workers. These systems are far from perfect and some of them are responsible for more dissatisfaction than any other single factor in the working environment. It is important to know what the worker feels and thinks about incentive payment schemes and further research is urgently needed.

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DESIGN OF PSYCHOLOGICAL EXPERIMENTS

By ROBERT H. THOULESS

The problem of design in experimental work includes such questions as:

(1) How many kinds of experimental observation are to be compared?
(2) How many experimental subjects are to be used?
(3) How many observations are to be made with each subject?
(4) How are the experimental observations to be arranged in time, e.g. (*a*) the degree of homogeneity or heterogeneity within experimental occasions, (*b*) the method of randomization of heterogeneous experimental observations?
(5) The method of statistical treatment of the data.

These depend obviously on one central question: 'What is the experiment intended to find out?' The best design of experiment is that which most economically answers the question with which the experiment started. Inferior quality of design is often the result of uncertainty as to precisely what one is trying to find out. Experimental instructions given to students may encourage inferior design by indefiniteness of the initial question. 'Investigate the so-and-so . . .', for example, invites uncertainty as to what is the question to be answered. 'Find out by how much the so-and-so differs under condition A and condition B (both exactly specified)' exactly formulates a problem and invites good designing of the experiment. The ideal of a well-designed experiment should be to begin with a clearly formulated question and to end with an answer to that question (or with a statement that the experimenter has not been able to answer the question). The remainder of the experimental report should be an account of a series of steps which enable the question to be answered.

One can hardly overestimate the importance of the contribution to problems of experimental design made by Prof. R. A. Fisher whose book *The Design of Experiments* (1) is of outstanding value to psychologists as well as to agriculturalists. At the same time we must, of course, bear in mind that there are differences between the problems of agricultural and of psychological experimentation.

I think there are two broad general principles of special value that the psychological experimenter can extract from Fisher's work. First, that it is not necessary to follow the old principle, once regarded as basic in the theory of experimentation, that one should vary only one causal factor at a time. It may be more economical to vary several factors at once while adopting methods of randomization and of statistical evaluation which will enable one subsequently to separate their effects. Secondly, that one should not first carry out an experiment and then think how to apply statistical methods to extract a conclusion; the statistical methods should rather be regarded as an integral part of the experimental design to be thought out at the same time as the other problems of designing the experiment. The use of *ad hoc* methods of statistical evaluation for extracting results from data not designed to be treated in that way leads to a good deal of inefficiency, loss of information, and even to wrong conclusions.

I think it is no exaggeration to say that the design of a good many experimental psychological researches leaves much to be desired. One often reads theses that suggest that the way of carrying out the inquiry has only started becoming clear to the experi

menter as he went along and has sometimes not become fully clear by the end. A few days spent in initial planning of the experiment may save months of fruitless work afterwards. I am not suggesting, of course, that these days should be spent altogether in the arm-chair (or alone). One can best think out problems of design in contact with the actual experimental work, and there is great value in exploratory experimentation during which the design is worked out. But this should be a definitely preliminary stage at the end of which the design should have been worked out clearly though not necessarily finally. The experimenter should, at the end of this stage, be able to say how many subjects he will use, under what conditions they will work, and how the results will be treated statistically. He might, of course, make modifications in the design afterwards but he should now know its general plan.

One (although not the sole) criterion of superior experimental design is the principle of economy. This is the principle that the maximum amount of information should be obtained with the minimum expenditure of time. There is obviously no sense in making 1000 experimental observations if the information required could have been obtained from 100 observations. Very often by altering the design of an experiment (e.g. by more efficient randomization or by substituting multi-columned correlation for two-columned correlation) 100 observations can be made to yield as much information as would otherwise be given by 1000 observations. When considering economy, however, it must not be forgotten that time is also spent in statistical working out of the results. A more complicated method of working out may effect an economy in experimental time, but a point may be reached where the saving of experimental time is more than offset by the increase in time of working out results. There is then no true economy. A very good example of a real economy in working-out time is the substitution of methods based on matrix algebra for factor analysis problems instead of the older method of working out thousands of tetrad differences. This has had the result that a table can be analysed in a few hours that would have taken months by the other method.

The idea of improvement in the design of psychological experiments suggests to many experimenters the use of the method of analysis of variance. The use of a design embodying this method may certainly sometimes be an improvement, but one must avoid a superstitious attitude towards the analysis of variance as if this were a method to be adopted in all cases without regard to the special requirements of the problem to be solved. For some problems it is the appropriate method, and where it is the appropriate method the patterning of the experimental design so that it may be treated by analysis of variance may effect a considerable gain in economy, enabling more information to be extracted from a smaller amount of experimental data. A mere blind use of this statistical method for experimental data not collected in accordance with the pattern of analysis of variance design, or for some other reason not appropriate for this treatment does not, however, lead necessarily to any advantage. At worst, it may make the logic of the process carried out unintelligible both to the experimenter himself and to other people.

Treatment of data by analysis of variance is often referred to as if it were a method entirely different from the older quantitative methods (e.g. working out a correlation coefficient or determining the significance of a difference between two means) involving new and somewhat dubious assumptions. It is, I think, better regarded as a generalization of these two simpler methods. Essentially one is analysing variance either in working out a correlation coefficient or in determining the significance of the difference between

two means. In the first case (a correlation coefficient) we consider the data as forming N classes, each of two members, and we make an estimate of the ratio of that part of the variance due to causes acting alike on the two members of the same pairs (and differently on members of different pairs) to the total variance due to all causes producing differences both within and between pairs. In the second case (determining the significance of the difference between two means), we are considering the data as made up of two classes and asking whether the causal factors acting differently on those two classes produce a variance between their means significantly greater than the variance due to the remaining causal factors which also produce difference within each class.

In each of these cases there is a single principle of classification. But exactly parallel problems can be dealt with when one has more than one principle of classification. We may have, let us say, a causal system A, B, C, which is acting differently on the three classes A', B', C', while there is a cross-classification D', E', F' of which each class is acted on differently by the causal system D, E, F. The number of systems of classification may, of course, be larger than two. In such a case, the problem is as to whether there is a variance between the classes A', B', C', and also whether there is one between the classes D', E', F', which, being significantly greater than the residual variance, must be attributed to an effect of the A, B, C and of the D, E, F systems of causation respectively. The appropriate statistical method for determining this is the analysis of variance. When reduced in this way to its essential formal structure, it is easy to see that the question: 'Have the classes A', B', C', a significant variance in excess of that which can be attributed to the error variance?' is essentially the same as the more familiar question 'Are the means of the class A' and class B' significantly different?' only it is asked of three or more classes instead of two. Also it is not essentially different from a correlation question because it can be put in the form 'Is there a significant correlation between all the members of the same class?'. The difference from the more familiar correlation problem is simply that the classes no longer consist of pairs but of some number greater than two.

By having introduced multiple principles of classification into our design, we have, of course, introduced new questions that may be asked not only as to the effect of the A, B, C and of the D, E, F causal systems considered separately but of their interaction, e.g. whether the A, B, C causal system acts differently on the D' and the E' and the F' classes.

Let us suppose, for example, that we have reason for thinking that the least perceptible difference in intensity of a sound stimulus varies with the absolute intensity of the stimulus. We might begin by verifying this law by taking two absolute values of the stimulus and making a series of measurements with a single subject of the least perceptible intensity difference at each of these absolute values. If the difference between the means of these two sets of data were significant, we should conclude that there really was a difference for this subject between differential thresholds at different absolute values of stimulus intensity.

Alternatively, we could decide this problem by taking a number of absolute values of the stimulus and making two measurements of the differential threshold at each of these values, using a suitable system of randomization. Then a significant correlation between these two sets of measurements would be evidence of a real difference between just noticeable differences at different absolute values of the stimulus.

The second method would have given us the advantage of a greater range of verification of the hypothesis at the cost of a lower accuracy of estimation of the amount of the difference. We can combine the advantages of both methods by making a number of observations at each of a number of absolute values of the stimulus. Then we shall have a series of means, and an experiment of more complex design which is already a simple case in which the testing of significance will be most economically carried out by analysis of variance. It is true that, if we prefer it, we can test significance by taking all the means in pairs and comparing them in the ordinary way, but this will become laborious when the number of means is large (e.g. for twelve values it would need sixty-six separate calculations).

But now let us suppose that we also notice that there are individual differences in auditory acuity. We could investigate this by taking our experimental subjects in pairs, giving each a number of discriminations and seeing whether they showed a significant difference between their means. Again we should probably prefer to use a simple analysis of variance design and to make a comparison between a number of subjects greater than two.

But the experiment would gain both in sensitivity and informativeness if we tackled both problems at the same time, giving several subjects discrimination experiments with several absolute values of the stimulus. We should now have a design with a double principle of classification in the data. Our results would be arranged in a matrix in which the rows were results by different subjects and the columns were results of a single subject at different absolute values of the stimulus. The statistical problem would be to determine the variances of rows and of columns and to see whether they were significantly greater than the residual variance. In other words, it would be a design requiring to be dealt with by analysis of variance. This type of design is being increasingly used in psychological experiments with, I think, great advantages in economy and fruitfulness where it is appropriate and where it is used with full understanding of its underlying logic.

It is not always so used and its inappropriate use has been criticized by Mr Babington Smith (2). We must agree with the main contention of his paper, that one must not use factorial designs and analyses of variance without consideration of the peculiarities of the particular psychological data on which they are used. In fact, I should regard this as a particular case of a general principle that one should not use any experimental design without consideration of the special requirements of psychological data. But I do not think this objection is as heavy against the use of the analysis of variance as Babington Smith seems to suggest. He compared subjects' responses to different arrangements of arrows in the Müller-Lyer illusion, presenting all different arrangements in a randomized manner to each experimental subject on a single occasion. He found that the responses of his subjects to each arrow arrangement were affected by their occurrence as parts of a heterogeneous series of arrangements.

His real difficulty seems to me to be revealed on p. 265 of his article. Babington Smith does not seem to have made up his mind as to what question he wanted to answer. If he wanted to find out whether any difference between responses to different arrangements of arrow heads persisted when these arrangements were presented in a completely heterogeneous series, then I think his design was well adapted to answering that question. If he still found a significant variance between arrow arrangements, then this would

prove that the difference does persist in heterogeneous presentation. It does not tell him, of course, how big the difference would be if the presentation were made in homogeneous groups and for that a differently designed experiment would have been necessary. It could still have followed a factorial design, e.g. all of his subjects could have worked on three experimental occasions using only one arrangement on each occasion, some following the arrangement *ABC* on successive occasions, some *BAC*, and some *CAB*. The use of a factorial design does not dictate the degree of homogeneity on each occasion. Which design was used would depend simply on what question was to be answered. One cannot reasonably use a particular design and then blame the design because it does not answer some other question than the one for which it is appropriate.

When Babington Smith says 'It is difficult to see how to design experiments which can produce assessments of the main effects in isolation', I cannot agree that there is any real difficulty. One can always make a design if one can clearly state the problem. If he means an assessment of the naïve response to a single arrow arrangement with no modification due to the subjects having seen any other arrangement, the necessary design is obvious. Each type of response will have to be made by a separate set of subjects. Obviously this would not be an economical design because the residual variance would be large since it would include all the variance due to individual differences between the subjects. It follows that a large number of subjects would have to be taken before the variance due to difference in arrow arrangement would significantly exceed the residual variance. Nevertheless, it is a practicable design, and if the question is really that of the response to a particular arrow arrangement by a naïve subject who has seen no other arrangement, some such design as this must be adopted.

In all quantitative psychological experiments one is investigating the difference in some measurable reaction (i.e. its variance) introduced as a result of some variation in the causal factor under investigation. Always this difference is observed against the background of a residual or error variance which is due to the variation of all other causal factors affecting the data. The detection of the action of the causal factor under investigation and the accuracy of estimation of its amount both depend on a reduction of this residual or error variance due to other factors.

This is just as true of the calculation of a correlation coefficient or of the determination of the significance of a difference between means as it is of the more general method of analysis of variance. The reduction of this residual or error variance to a minimum can be done either by improving experimental conditions or by altering experimental design. Avoidance of noise and other distractions in the laboratory are, for example, methods of reducing residual variance by improvement of experimental conditions. The use of equivalent groups for a comparison of means is an example of improvement in experimental design directed to reducing residual variance, although it is often not realized that the advantage will be lost unless one also uses a method of statistical comparison which takes advantage of the resulting reduced variance.

Mr Babington Smith considers that his experiments undermine the 'tendency to regard reduction in size of residual error by itself as a sign of efficiency of experiment'. There seems to me to be no matter for controversy here. Obviously it is a gain in economy to make the residual error as small as possible, whether it is done by improved experimental conditions or improvement in design. Such an increase of economy is a good thing, if, and only if, this reduction in residual error is not attained at the cost of losing

some other object in the experiment. It may be necessary to design an experiment with a large residual error because a question is asked which cannot be answered by an experiment of somewhat similar design in which the residual error would be smaller.

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DO MODERN MECHANISMS HELP US TO UNDERSTAND THE MIND?[1]

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I. *Recent progress in mechanisms* (pp. 24–25). II. *Analogies between automatic control systems and body mechanisms* (p. 25). III. *The predictor and its mind-like functions* (pp. 25–28). IV. *The distinction between function and structure* (p. 28). V. *The function of recognition* (pp. 28–31). VI. *Mechanisms for counting, sorting and calculating* (p. 31). VII. *Machines for logical deduction* (p. 32). VIII. *The storage of information and memory* (pp. 32–33). IX. *Mechanisms that have spontaneous behaviour patterns* (p. 33). X. *A general survey of the relationship of possible nerve mechanisms to the mind* (pp. 33–35). XI. *The difficulties of the mechanistic view* (pp. 35–37). *References* (p. 37).

I. Recent progress in mechanisms

The recent developments in mechanisms, that are being invoked in current discussions of the relation between brain and mind, are mainly in the following groups:

(1) Automatic control systems, like the 'automatic pilot', that seem to simulate some human functions.

(2) Computors of the analogue type, like the 'predictor' used to direct gunfire.

(3) Devices for counting, and for automatically carrying out sequences of counting or combinatory processes, as in the digital computor.

(4) Sorting and indexing mechanisms, as in the Hollerith tabulating machines.

(5) Means for 'coding' information, as when a television camera codes a visual scene into a stream of electrical signals for transmission.

In the same period as these developments, the electronic amplifier has made possible new observations of electrical phenomena associated with brain activity, including electroencephalography and observation of the responses of individual neurones and small groups of neurones to the incoming trains of electrical pulses that constitute nerve signals.

These developments have naturally and properly stimulated renewed attempts to give an account along mechanistic lines of the working of the central nervous system, and to attempts to press this account further towards the inclusion of those phenomena commonly described as mental (2,1).

Unfortunately, speculation in this field has not always been either well-founded or cautious, and many laymen have been given the general impression that machines have unlimited possibilities, and the working of the brain is now understood 'in principle' in terms of machine-concepts. It is important to discriminate between irresponsible speculation and that careful forming of hypotheses and analysis of their consequences that may in time provide psychologists with valuable conceptions as to the manner in which the

[1] Summary of a lecture, given to the Midlands branch of the British Psychological Society, in which the author had been asked to consider whether the mechanisms of the brain may resemble the mechanisms of electronic calculators and automatic controllers. A limited number of diagrams must here replace the demonstrations of apparatus with which the lecture was illustrated.

mental phenomena which they study may be conditioned by the development and the structure of the brain.

One reason why the assertions both of 'mechanists' and of 'non-mechanists' in this field tend to go beyond the evidence is, of course, that the topic concerns the nature of men, and is not emotionally neutral. The non-mechanists fear mechanistic views as threatening to knock away some props of their morality. Mechanists, on the other hand, fear the removal of the strait-jacket of order and manageability in which their intellect has contrived to confine this dangerous world. A recognition that the controversy has emotional roots may make it easier to weigh the real evidence and to arrive at a just assessment.

II. Analogies between automatic control systems and body mechanisms

The question to be considered in this paper is the possible bearing of our knowledge of mechanisms on our understanding of 'the mind', i.e. on the subject-matter of psychology, as distinct from the subject-matter of neuro-physiology. In neuro-physiology the relevance and value of our knowledge and experience of engineering mechanisms is self-evident, so that machines do at least help us to understand the mind in so far as progress in neuro-physiology can be said to help us to understand the mind. The conviction that it can has good roots in the fact that mental events and physical events in the body and the brain are parts of the conjoint 'mechanism' of the organism's behaviour. They are parts of a common whole.

To see more clearly the way in which mental events are related to body mechanisms it is illuminating to put side by side the scheme of elementary functions and the physical elements associated with them, on the one hand for a human action, and on the other hand for an analogous action of a physical mechanism.

An example is illustrated by Fig. 1, which indicates the chief elementary functions involved when a boy throws a stone at a bird, set alongside the analogous mechanical arrangements by which a shell is aimed from a ship at an aircraft target.

Because the general function is similar, i.e. throwing a missile at a moving target, it is not surprising that the subsidiary functions are closely similar in form. The radar aerial performs the functions of the eye, electric cables those of the nerves, motors those of the muscles, and so on. The analogy holds in considerable detail, and the study of such analogies has given stimulus and assistance to physiologists in the understanding of such matters as the part played by proprioception, the possible significance of the temporary over-response of receptor organs on initial stimulation, and so on. These are problems in mechanics. Our present purpose is to look for the possible analogy with more specifically mental functions.

III. The predictor and its mind-like functions

Suppose one leave aside for the moment (not neglecting its importance) the question of *why* the stone was thrown or the shell aimed, which are aspects of a larger situation, and look only at the activity itself. One notes that both in the gun control and in the stone-throwing boy there is a connecting link between the 'sensory' elements and the 'motor' elements. In the gun control this is the predictor, in the boy it is such elements of the brain (or anything else) as are involved in the chain of causation between events in the visual projection areas and events in the motor areas.

The function of the predictor in gun-aiming is a limited and specific one. It is to adapt the aim of the gun to certain variable features of the external situation, namely the direction and speed of movement of the target, and particularly to modify the motor-activity so as to 'aim-off' by the right amount in the right direction so that the shell meets the path of the target, allowing for the time the shell takes to travel.

It is suggestive and illuminating to examine rather closely just how the predictor performs this function.

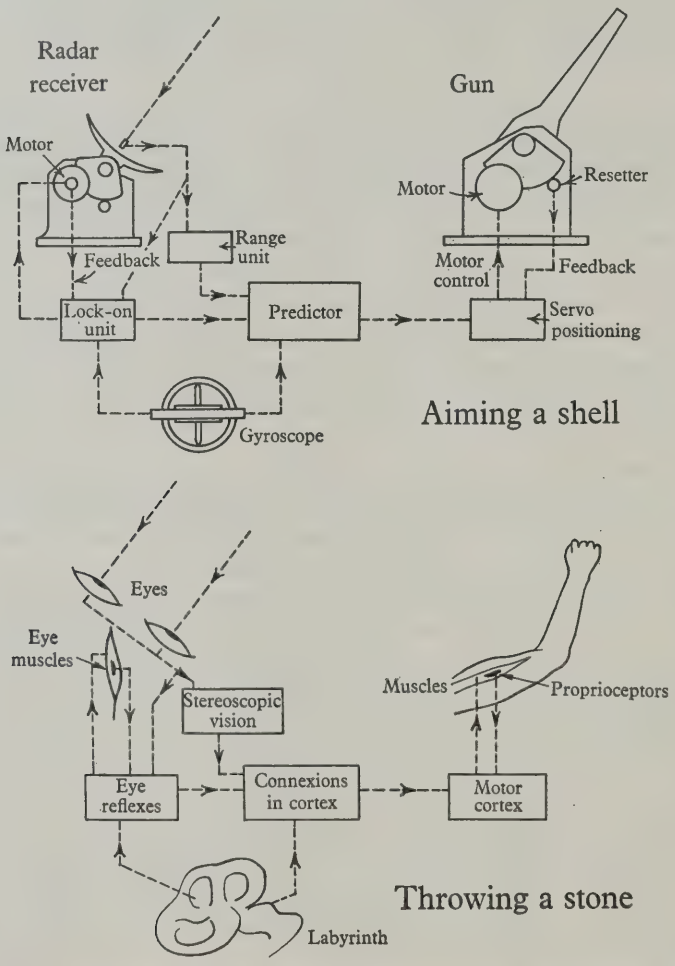

Fig. 1. A rough schematic comparison of the elements involved in an analogous operation by a mechanism and by a human being.

Predictors, like boys, have evolved, and it is possible to trace their development from very simple devices to the elaborate modern types. The main principle by which their structure provides for their function is most clearly seen in the simpler types, but it is essentially the same in all of them. The principle is that the predictor is so constructed that its essential elements embody (or constitute) a kind of image or simulacrum of the relevant features of the external situation. In the very simplest predictors this simulation of the external situation is only a triangle of steel rods with hinges and slides as shown in

Fig. 2. This constantly duplicates (in accordance with clues from the 'sensory' elements) the triangle whose corners are the gun, the target, and the future target position, the rods forming a similar (or affine) small-scale representation of this triangle. The predictor is, so to speak, a moving diagram. The representation is continuously kept in correspondence with the relevant external situation by 'sensory' data, and it continuously determines, through the motor mechanism, the line of aim of the gun.

In some more modern types of predictor one would look in vain for a moving triangle. It is there, but it is invisible. The lines of the moving diagram have been 'coded'. For example, the length of the lines may be represented by the magnitude of alternating currents, and their directions in space by the directions of the magnetic fields that these currents set up in electro-magnetic 'resolvers'. The moving diagram has been transposed to another medium: a pattern of relationships exists corresponding with the space-triangle of the external world, but this representation is not 'in space' in the same sense.

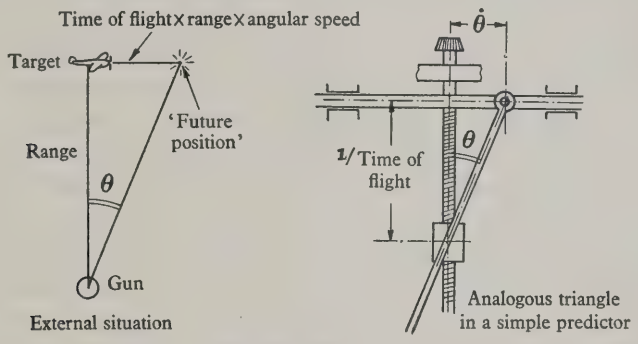

Fig. 2. Diagram showing how a predictor is essentially an analogue of the external situation.

We must now turn our attention from the predictor to the stone-throwing boy, and ask what it is that in this case mediates between sensory stimuli and the direction of motor activity. An important part of the answer is 'the boy's awareness of the external world'. A full answer would be more complicated, and functions are involved, that will be considered later, that have no analogue in the predictor. The term 'awareness' is used here in the ordinary sense of part of mind, begging no questions about what physical events in the cortex may also be involved.

It is very striking that awareness can be described in words almost identical with those that describe the essential nature of the predictor. Awareness is a changing pattern corresponding in biologically relevant ways to the external world on lines determined by inborn structure and potentialities, but determined, in its particularity at any time, by sensory clues, and serving to adapt motor activity to the changing external world.

Of course, there is nothing in this comparison of the mind with a predictor that is in itself an argument for the mechanistic view. The analogue structure, in the only aspect of which we are at present aware, is a structure of thoughts and ideas, part of our stream of consciousness, though there is some evidence that processes analogous to those of consciousness can go on outside of consciousness, even to the solution of mathematical problems. It is difficult, however, to be sure that this is a case of genuine absence of consciousness or merely of absence of memory of the process. The mechanistic hypothesis is that these events in consciousness are accompaniments or aspects of physical events in the brain. This may be true, and in that case there would exist, in the physical structure

of the brain, changing patterns of some sort, corresponding in some coded form with our idea of the world around us, and therefore corresponding, somewhat more remotely, to relevant aspects of the actual external world. It may well be that such structures exist, and even that we shall one day find them, but the comparison with the predictor is no evidence on this point. It would be valid for a philosophical idealist as well as for a mechanist.

This aspect of awareness is not however all we mean by mind. It will be convenient to return to this topic later, after considering other 'mental functions', but before leaving Fig. 1, however, there is a general feature of such parallels between mechanisms and human beings to which attention may be called.

IV. The distinction between function and structure

A general inference from Fig. 1 is that such comparisons warn us at almost every point that we must not infer similarity of structure, or even of agency or means or medium, from similarity of function. One may take illustrations at random. The nerve-fibres of the boy have the function corresponding with that of the electric wiring of the mechanism. They even have a certain structural similarity, such as their insulating sheath. But no electric currents flow along nerve-fibres. The signals along them are discrete self-propagating local electro-chemical discharges, like minute smoke-rings of chemical charge propagated along the periphery of the fibres. Or consider, as another example, the gyroscope in the mechanism and the corresponding structure—the labyrinth or semicircular canals—in the boy. The functions are the same. A stone or a shell must be thrown with appropriate reference to the direction of gravitation; the rolling of the ship or the swaying of the body must be compensated for. The external world must not seem to tilt when the boy tilts his head. This reference of activity to the gravitational vertical is the function of the gyroscope in the mechanism and the labyrinth in the boy. In flying insects a similar function of reference to fixed planes is performed by their 'halteres' or vibrating hairs, and in other organisms by a 'marble in a bowl' type of structure. Here are four different structures performing similar functions.

As a further example, gun-control mechanisms as indicated in Fig. 1 can be constructed alternatively in the media of electric, hydraulic or pneumatic elements, or various combinations of such elements. The functional scheme shown would equally correspond with any of these. Any inference of either structure or medium from similarity of function must always be made with extreme caution. We have so far been concerned with events in a 'mental medium'.

Some other mental functions and some types of mechanism that are not specifically included in the example of Fig. 1 must now be considered.

V. The function of recognition

The term 'recognition' is used of mechanisms as meaning a process in which particular stimulus patterns result in specific or distinctive responses. As applied to man the term is also used for the identification of a stimulus pattern with something already in memory or as classifiable under an appropriate general concept or name, and the appearance of the concept or name in consciousness. There is a closely associated process, that we would not normally call recognition, when we are subjected to some novel stimulus-pattern, and

form an 'idea' from it, i.e. we in some way distinguish, and index, those distinctive clues by which we shall in future recognize the same thing again, or classify somewhat similar things under the same name.

It may help in appreciating the remarkable nature of the function of recognition in man to consider some of the simpler types of mechanism that perform the function of recognition in the strictly limited sense of giving distinctive responses to particular patterns of stimulus of input. Such mechanisms are common and familiar. In this sense the lock 'recognizes' its key, the Hollerith card sorter will pick out those cards from a pile that are punched with various particular patterns of holes, and so on.

It is very easy, for example, to make mechanisms that recognize number. Fig. 3 shows the circuits of a simple demonstration model, using 5 relays, that will display the corresponding number when, of the 5 keys, either 0, 1, 2, 3, 4 or 5 of them are pressed, independently of which particular keys are selected. The model may be said to recognize numbers up to 5.

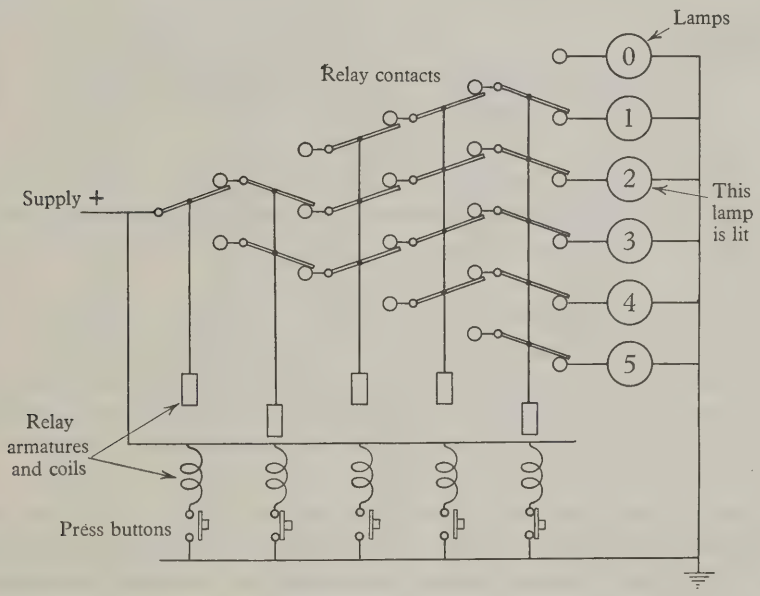

Fig. 3. Demonstration model of a mechanism that 'recognizes' numbers (up to 5) (shown recognizing the number 2).

It is easy to contrive an electrical 'combination lock' that will respond only when one particular combination of a number of circuits is 'live'. Fig. 4 shows the principle, for 5 lines. The output circuit is activated if and only if the particular combination of lines are 'live' that corresponds with the pattern of connexion through the upper and lower contacts. Five wires provide 2^5 or 32 combinations. Twenty would provide 2^{20} or about a million combinations, and so on. The same arrangement, extended to a million sets of 20 relays all differently wired, could 'recognize' each of the million different possible combinations obtainable by, say, pressing any combination of a set of 20 keys, in the sense that each combination would excite one and one only of the million outgoing circuits.

A 'relay' for such purposes need not of course be constructed like the telephone-style magnetic relays the engineer uses. Any device will do which has two distinctive states

(corresponding with conducting and non-conducting positions in the case of the relay, say in general X and not-X) and in which an X-state input determines either that the output will be X state or that it will be not-X state. The neurone, or group of neurones, possesses properties by which it could conceivably function in such ways. It can be 'firing' or 'not-firing' and the firing or non-firing of one neurone may either facilitate or inhibit firing in another. It should be noted, however, that a sustained stream of pulses is required to maintain the activity, a single pulse does not suffice.

That it is easy to imagine 'neurone nets' with the properties of the nature of a combination lock underlies the common assertions that the human capacity for recognition, and the associated phenomena of 'gestalt perception' or recognition as wholes, and even our capacity for conceptual thinking, are *compatible* with the mechanistic hypothesis (2,1). Attempts to carry the hypothesis further towards confirmation or rejection have raised many interesting speculations, and have resulted in a number of significant new observations, but are far, at present, from any positive result.

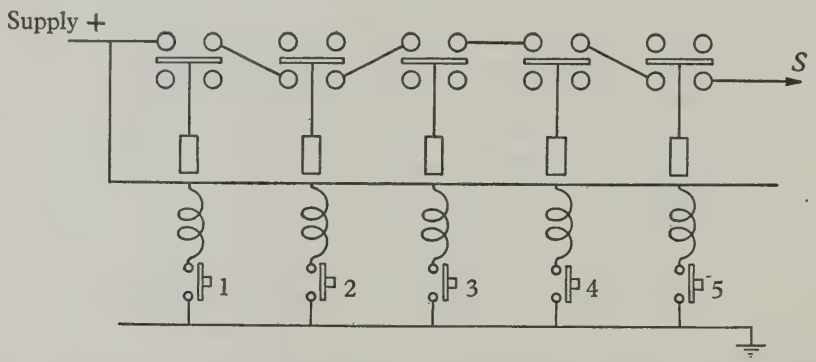

Fig. 4. An electrical 'combination lock'. The line S is 'live' if and only if a particular combination (in this case 2 and 5) of the buttons is pressed.

To an engineer, considering the possible construction of a mechanism that would perform analogous functions, the human function of recognition is astonishing in its achievement. Consider, for example, the recognition and naming of a person one met a year ago. Almost instantly one says 'Bill Jones'. Yet, the visual pattern on the retina that provoked that response was only in almost indefinable details different from the ten thousand other faces one has recently seen. The particular pattern seen, is not, in fact one ever seen before. A face has innumerable aspects, distances, lighting and background. Its image may fall on various areas of the retina.

The theoretical notion has been advanced (5), to account for visual recognition, that a pattern of signals corresponding with the image on the retina is suitably centred, enlarged or diminished in size, and rotated through the correct angle so as to be brought into coincidence with memory traces, and when there is substantial coincidence, the bell of recognition rings. This conception may perhaps be made clearer by a simple analogy. Suppose one had a lantern-slide (positive) and wished to find from which of a pile of rather similar originals (negatives of various sizes) it had been made. One could put the slide into a projector having a point source of light, and project the picture on to a screen. Now let the negatives be taken, one by one, and moved about in the beam. If the negative

is the one that corresponds with the positive slide there would be some one position in which the negative exactly coincided with the projection of the positive, and the screen behind would be uniformly darkened for that one position and for no other negative nor any other position. This darkening would correspond with 'recognition'.

There seems no likelihood that human recognition, say of a particular face, is achieved by any such 'template-fitting' process. Everyday facts suggest that the principle is more like what might be described as a cartoonist's filing system. It seems that we remember and identify, not from some superposition and coincidence principle, but by indexing from a limited number of clues. We recognize a cartoon, and perhaps experience pleasure in so doing, because of the isolation and enhancement of the clues. A hat brim, a cheek-line and a cigar can unmistakably say 'Churchill'.

In a quite different field, consider the game of twenty questions. It is astonishing that twenty yes-or-no answers can usually identify any nameable thing within the knowledge of the interrogator. The possibility well illustrates the part played in recognition by something like a classification index and a sorting mechanism, and it is indexing and sorting at most astonishing speed and efficiency.

VI. MECHANISMS FOR COUNTING, SORTING AND CALCULATING (6)

Perhaps of all mechanisms the class that has struck the popular imagination as being most 'brain-like' is the high-speed digital computor, which the newspapers insist on describing as the electronic brain. This analogy seems to have little basis, for the following reasons.

The assertion of similarity is usually based on the fact that both nerve fibres and the circuits of calculating machines carry trains of pulses. The significance of these pulses is, however, quite different. The calculating machine works by counting pulses. It depends absolutely on an unfailing specific response to each single pulse as such (normally the reversal of a thermionic 'flip-flop'), and the new condition is maintained until the next pulse arrives. There is no action of this kind directly available in the usual response of the neurone, where commonly a number, but not a specific number, of incoming pulses are required to cause it to emit a pulse. A neurone can respond to the near-coincident arrival of pulses or short trains of pulses, by distinct channels, by itself emitting a pulse. In this it may be well suited to provide mechanisms of the indexing, sorting or recognition type, but not of the counting type.

Secondly, it is mere mysticism to attribute, without explanation, general properties to an electrical computor that cannot be attributed to simple, indeed almost trivial, mechanical devices. There is a progressive series of devices from the cyclometer that counts the revolutions of a bicycle wheel, the desk calculator that counts by ratchet and pawl, the electrical calculators that count by opening and closing relays, to the modern machine that uses valves that go alternately conducting and non-conducting with each pulse, and so count the pulses.

Thirdly, even when we do mental arithmetic, we do not normally do it by processes like the sequences of events in digital computing machines.

The conclusion must be that the detailed processes that are specially characteristic of digital computors are not likely to be enlightening in respect of the working of the brain.

VII. Machines for logical deduction

A somewhat more plausible parallel with brain function may at first sight seem to be provided by those actual or possible machines that indicate the result of applying a fixed set of rules of manipulation. This class includes the machine that plays 'noughts and crosses', machines that operate in correspondence with the rules of formal logic, and so on.

In the 'noughts and crosses' machine each situation (i.e. each possible combination of noughts and crosses at any stage) defines one or more 'correct' plays, which depend in accordance with simple rules on the disposition of noughts and crosses at the time. If a nought or a cross in a given square is made to correspond with one or other of a pair of wires being 'live' it is a simple matter to arrange a set of relays in such a way that the operation of the relays makes a lamp circuit that lights up the square corresponding with the application of the rules for correct play.

The so-called logical computors(3,4) depend on similar principles. For example, an electric wire can have the two states, 'live' and 'earthed'. These may be identified respectively with 'proposition true' and 'proposition false'. Suppose a set of wires be identified with a series of propositions. Then various types of relationship between propositions can easily be simulated by simple types of electrical connexion. For example, the relationship 'If A is true then B is true' requires a device (of the nature of a connexion through a rectifier or other one way device) that makes the B line 'live' if A is live. The relationship 'if A, not B' is represented by a relay excited from A so as to earth B whenever A is 'live', and so on. A combination of such interconnexions corresponds with the simultaneous existence of the corresponding relationships between propositions, and the result, i.e. whether a particular wire (identified with a particular proposition) is 'live' or 'earthed' necessarily corresponds to whether the proposition is 'true' or 'false' in the sense of being a result of applying the specified relationships to the assumptions. The 'assumptions' are of course represented by making 'live' the wires identified with the propositions that are taken as assumptions.

There is no reason to suppose, nor has any responsible person suggested, that there is any evidence to suggest essential analogy between such devices and the mechanisms of the human brain.

VIII. The storage of information and memory

Storage and recall of information, corresponding with memory in man, may be introduced in a great variety of physical forms, such as records of pulse-trains on magnetic tape or drums or films, or records on punched cards. Another group of 'memory devices' is of special interest because it involves a trace that is in itself evanescent, but which is periodically renewed for so long as it is required. Such evanescent-but-self-renewing records have been used in digital calculators in two forms. In one the record is a distribution of electric charges on a screen like a television screen. The second form is a train of sound pulses circulating round a tube filled with mercury. Both of these have been made to work with such precision that a signal rewritten many times each second may be perfectly preserved over hours or days. Such systems are referred to as reverberatory chains. It has not failed to be suggested that human memories may, at least in part, be stored by such 'reverberatory chains' of neurones. For this there seems to be no particula

evidence. The facts that have to be accounted for are extremely baffling. For example, the work of Lashley has shown that conditioned responses to visual stimuli seem to depend on storage wholly in the visual projection area of the cortex, that all parts of this area are capable of memory, which does not appear to be localized, and the same elements are probably used in normal vision. Lashley suggests some sort of 'patterns of resonance' as the seat of memories. Others have suggested patterns of enzyme structure in nerve cells. One name for the unknown seems as good as another!

IX. Mechanisms that have spontaneous behaviour patterns

The emphasis that used to be laid on reflex activity and on the conditional reflex in animal and human psychology has somewhat given way during recent years to greater emphasis on the essential part played by innate behaviour patterns, that arise from inbuilt structure. This factor, so easily recognizable in such specific behaviour as unlearned nest-building in birds, has a rather different character in man, where the rigidity of pattern is lost and the versatility of behaviour enormously increased by an elaborate mechanism of a different kind, namely the structuring of motivations, largely subsequent to birth, under the impact of the social tradition. This involves the elaborate mechanisms of the formation of conscience (the super-ego), the use of symbols and mythology as a vehicle and reinforcement of dominant patterns, and the rest of the apparatus that psycho-analysis has begun to reveal. These complications condition the forms of activity, but the spontaneous character of activity is fundamental. We try to express it by the use of terms like drives, urges and the like.

It is easy to cite examples of mechanisms that go through a sequence of behaviour, when appropriately stimulated, and where each stage follows the completion of a previous stage. The record-changer of a gramophone or the automatic capstan lathe are simple examples. It is also easy to find, or to contrive, mechanisms that 'seek out a goal', like Dr Grey Walter's 'tortoise' that, when it became 'hungry', i.e. when its battery was running down, moved back to its hut and plugged itself on to the battery charger. Mechanisms that function like primitive conditioned reflexes can also be constructed.

These toys have value in warning us that it is easy to attribute mind to mindless things, but they also emphasize that behaviour ultimately depends upon inbuilt structure, and previous state, and may depend only secondarily upon external stimuli.

X. A general survey of the relationship of possible nerve-mechanisms to the mind

Modern developments in physical mechanisms have tended to make more credible the mechanistic view that human behaviour may be a result of an immensely complex, but potentially understandable, network of nerve fibres and synaptic junctions, together with certain hormonic mechanisms via the blood stream.

The objections usually raised against this view seem to the writer to miss its real shortcomings. The kinds of questions asked are 'Could such a mechanism compose a sonnet?', 'Could it solve a mathematical problem that required a mathematical method not previously learned?', or 'Could it have moral conflict?', and so on. Such objections relate in one way or another to the fact that such a mechanism would have to be very complex indeed to correspond with the versatility, flexibility and scope of human

behaviour. A self-building, self-sustaining mechanism of such complexity would indeed be astonishing, but so are many aspects of the universe. Our astonishment is scarcely evidence.

One can sketch the general lines of the mechanistic scheme that would be required to account for human behaviour, and it seems possible to show, item by item and function by function, that there is no aspect of human *behaviour* (as distinct from experience) that is of such a nature that it immediately and obviously falls outside the scope of mechanisms conceivable 'in principle', however hopelessly beyond the scope of possible human construction.

To complete such a theoretical scheme requires additions to those potentialities of mechanisms that have so far been mentioned. An important gap, to fill which no existing mechanism can be cited, is the formation of abstract concepts. It is possible, however, to incorporate this function in a hypothetical mechanistic scheme based on the conception of a sustained inner representation or simulacrum of the external world of the kind that was mentioned earlier.

It may help, in order to focus the present status and the shortcomings of the mechanistic view, to sketch briefly the kind of account that would be given, from this point of view, of the field of events, physical and mental, with which psychologists are concerned. This might be attempted along the following lines.

An essential mode of functioning of the human organism is to build up an internal analogue or simulacrum of the external world. The general lines on which this occurs depends upon innate structure, but the outcome depends also upon the individual history of sensory clues from which the simulacrum is constructed. The construction is in some coded form, the nature of which is at present unknown. The simulacrum is only at present accessible to the extent that we assume that it is in correspondence with our subjective experience, as our knowledge and awareness of the external world. The process of building this inner representation corresponds with the 'latent learning' of animal psychologists. The biological function of this inner analogue is to promote better adapted behaviour than is possible with only reflexes, conditioned reflexes and trigger-mechanisms.

This simulacrum or analogue of the external world is not (as in the predictor) transient and dependent only on contemporary sensory clues. It is a permanently growing structure, constantly elaborated and stored as our *idea* of the external world. It has, moreover, independent activity (corresponding with the subjective experience of thinking) when overt action is suspended and there is a process of inner experiment on the analogue exploring the possible outcome of 'imagined' action, probing, as it were, into possible futures. The motivational mechanisms are activated, not normally from the sensory stimulus directly, but by the inner construction and its predictions forward in time. The fundamental importance of conflict in man arises necessarily as a result of this replacement of direct response to sensory stimuli by the suspense of action and the probing of alternative futures, until a combination is found that is decisive for response.

This process that we call thinking, occurs in terms of simplified representational elements, symbols, 'concepts' and names, that are not the substance of immediate awareness, but are related to the classification and recognition mechanisms of memory and recall.

This mediation of activity by inner experiment must be understood as holding up and temporarily suppressing spontaneous and reflex activities that are actively present

the sense of a pressure towards activity. The organism is dynamic, and the condition of dynamic activity dammed back, regulated or suppressed, corresponds with subjective experience of desire, aspiration, resolution, and the play of emotional tone generally. Free-will is a term referring to this possibility of suspending overt activity until motivation is released.

An account on these lines might be much further elaborated, and it is evident that given the basic elements of spontaneous patterns of behaviour, and elaborations corresponding with the formation of concepts, learning, memory and choice, a plausible case may be made for the proposition that a mechanism is *conceivable* that would write sonnets or discover the theorem of Pythagoras, and that the propriety of the mechanistic hypothesis should not be denied on the grounds of the complexities and subtleties of human behaviour.

XI. The difficulties of the mechanistic view

There are, however, more formidable difficulties. They relate to our experience of consciousness and sensation. It is easier to conceive of an automaton completely simulating human behaviour than to conceive of any physical mechanism, as such, having even the most primitive semblance of the consciousness that we each experience. This difficulty might ultimately be explicable, but it is a very deep-seated one.

Of course behaviourists avoid the difficulty by the simple expedient of omitting from their account all mention of the existence of the stream of thought, feeling and sensation. But psychologists are certainly concerned with thoughts, feelings and sensations as well as with behaviour, and it seems very odd to begin by excluding them from consideration. Quite apart from this, if one were concerned only to understand behaviour, it seems strange to neglect the one privileged access available to such immediate precedents and accompaniments of behaviour as thoughts and feelings, that are part of the complex of events of which behaviour is also a part. Even the most thorough-going mechanist, who would say that our thoughts and feelings are merely, so to speak, the 'insides' of an essentially mechanical process, would say also that every idea, recollection or emotion corresponds with events in the brain. In that case, in an attempt to understand these mental events, what sense is there in putting out of court the detailed clues provided by the sequence, relationship and apparent causal concatenations of events in our consciousness? These are evidence, at the least, that there are corresponding sequences of various distinguishable kinds of physical event.

The main difficulty of the mechanistic view lies in the entire absence of any link between the hypothetical physical system and the events of consciousness.

The usual escape route from this difficulty is to evoke the word complexity. The predictor, it is said, is a very simple mechanism, and so not conscious. The brain, although in essentials similar to a predictor, is extremely complex, and so it is conscious. This is no approach to explanation. A machine, however complex, is merely a bit of the physical world, and cannot legitimately be endowed with entirely new properties because we call it a machine.

Another escape route is to evoke the principle that the whole may show phenomena not shown by its parts. Certainly it may, but this is no contribution to removing the complete lack of any idea at all of how this particular whole could conceivably produce *these* particular phenomena.

It is noteworthy that many engineers who are on familiar terms with feed-back control systems, relays and calculating machines, and who devise and construct them, do not accept without protest the disposition that some non-engineers are showing light-heartedly to throw in sensation and consciousness as epiphenomena somehow associated with certain physical mechanisms. The physical phenomena in a system of relays or of thermionic valves are not different in nature from those in constructions of push-rods and cog-wheels. They are ultimately systems of particles. If less familiar physical concepts are invoked such as enzyme patterns at cell surfaces or electrical field distributions, resonance patterns and the like there is nothing essentially new. These things are just the common phenomena of the physical world, they are not somehow different so that they can arbitrarily be made to carry a special load just because the bit of the physical world in which they occur happens to be a calculating machine or to be some hypothetical robot of great complexity, so that we can then arbitrarily throw in 'mind' as an extra.

Up to some advanced stage in animal life, evolution may well have resulted in elaboration of the mechanisms of adaptive behaviour of the kind that corresponds with the mechanist hypothesis. We do not conceive of such mechanisms as conscious. Any presumption that these mechanisms as such imply consciousness might well lead us to suppose that a predictor or an automatic pilot is also conscious. The fact that at some stage the mechanism becomes elaborated in the direction of evolving a sustained inner representation of aspects of the external world does not of itself imply consciousness. This is present in every analogue device. The further development that a representation of the device itself appears in the inner analogue (i.e. self-awareness) does not alter the situation at all. The mechanist's robot is a chronic sleep-walker. Something else has appeared in the course of evolution, namely consciousness. It is true that there is no evidence whatever that consciousness is not widespread in the physical world. If it were we should be unaware of the fact, for we experience consciousness only individually and infer it only from behaviour like our own in circumstances in which we ourselves are conscious. Conversely, the fact that a cunningly contrived robot might result in our thinking it was conscious when it was not conscious, and even satisfy all the possible tests we could contrive, does not make the question of whether the robot is in fact conscious a meaningless one, for the simple reason that there is one at least of the human population that I am quite certain is conscious, namely myself. I can scarcely be satisfied with a scientific account that is only consistent either if I am never conscious or else if certain mechanisms as such are conscious.

Such considerations have of course been familiar to philosophers for hundreds of years. The development of modern mechanisms does not alter or remove the essential difficulties.

Our experience of ourselves and machines seems to leave two possibilities. The first is that physical being, as such, can imply consciousness, which would be difficult to disprove but which is remote from our habitual assumptions. The second is that the phenomena of consciousness constitute a distinct field of phenomena which interacts in comparatively rare situations with material phenomena, i.e. our conception of mechanisms must be extended.

Electrical engineers may perhaps be disposed to the latter view, for their own field of interest presents an interesting analogy for such a situation.

Not many decades ago a man put together for the first time that particular pattern of pieces of iron, copper and cotton-rag that constituted a dynamo, and when this speci

mechanical object was rotated a new field of phenomena became apparent. The dynamo became, as we say, electrically alive. It revealed *electrical* phenomena, rarely exhibited, and never previously recognized anywhere else. We now know that a variety of interrelations exist between particular structures of 'mechanical' parts and the field of electric phenomena, and these mechanical parts themselves must now be understood as in some sense more primarily electrical than mechanical, so that ultimately the two fields are one.

Is it conceivable that just as electrical phenomena were unrecognized until a particular structure of mechanical parts showed interaction with them as something that could no longer be neglected in our account of reality, so evolution, in the elaboration of the kind of brain that corresponds with the mechanist conception, chanced upon a rare linkage with phenomena of a different kind, and turned this to biological account? If this speculation should happen to have resemblance to the truth it would give grounds for hope that patient investigation may ultimately give us some understanding of these phenomena, although such an account would not be wholly in terms of the concepts of present-day physics. This goal is at present a very distant one.

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THE OBJECTIVE STUDY OF MENTAL IMAGERY

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I. Introduction

Images are the materials of thought, and the mode of thinking a person habitually employs may well depend on the particular type of imagery at his disposal. Roe (7) has suggested that characteristic variations in imagery are to be found in people in different occupations. Images are least often reported, though not absent, where the subject-matter is predominantly 'abstract' or non-sensory. For instance, theoretical physicists 'rely heavily on symbolic thinking, which is related to, but not strictly, verbal thinking in auditory or auditory-motor terms'. Even the Wurzburg group acknowledge that 'sensory presentations' (images) are 'the most obvious feature in the subject's reports' (Marbe). Moreover, if, following Craik, we hold that images symbolize modes of possible actions, or are 'working models' of actions (whether the latter belong to the past, as in reverie, or to the future, as in planning a journey), Marbe's 'Bewusstseinslage' may be taken simply as another way of expressing the *way we manipulate* our imagery-models.

In view of the part played by images in thinking, imagery questionnaires are poor indicators of the function of images in mental life. They fail partly because individual's self-scorings cannot be standardized, and partly because, in thinking, it is the images that occur most readily and habitually that are important, not the ones thought to be the most 'intense' or 'vivid' at a given moment. The mere emergence of very vivid images may not be associated at all with the tendency to have and to use images. In the present study, mental tasks were given to subjects, and spontaneous reports obtained from them directly afterwards. These data were used to determine imagery-types.

The hypothesis adopted, therefore, is that images contribute to the models or schemata we create of the world, just as much as 'direct' seeing, hearing or speaking. Accordingly the following operational definition of an image is used for the present inquiry: 'An image is a sense-experience in one or other of the sense modalities which can only be distinguished from a percept, hallucination, or illusion, according to (*a*), the context in which it occurs and (*b*), the attitude of the experiencer, including his ability to construct it.'

This definition makes no rigid distinctions between the types of awareness named. When Ryle (8) distinguishes between the metaphorical use of 'imagining' words and the literal use of 'perceiving' words, urging, in consequence, a complete dichotomy between imagining and perceiving, then he is arguing from an observed difference in logico grammatical usage to no more than the hope of a difference in psychology—a hope so far unfulfilled experimentally. Sartre (9) offers a similar argument, saying that the 'subjects could not have meant to say that the images are given to them visually. Nor do they imply that images occur as neural processes or processes of visual centres'. This

leads to the dictum, for which there is no experimental evidence, that 'the image represents a certain type of consciousness which is completely independent of the perceptual type'.

On the contrary, the interdependence of imaging and perceiving, well established by Bartlett in *Remembering*, and the several experiments quoted by Woodworth (11), showing that an image may be readily confused with a percept, encourage one to look for a *common* physiological source for both types of awareness: for speaking or verbalizing, respiratory movements; and for seeing and visualizing, the electrical activity of the visual association areas of the cortex. An examination of these physiological factors constitutes the 'objective' study of this paper.

II. Earlier investigations

(i) *Respiration and imagery*

Pioneer work was undertaken by Golla & Antonovitch (2), who measured the respiratory rhythm whilst subjects were (*a*) unoccupied, (*b*) engaged in certain mental tasks. Two types of respiratory behaviour were noted, and they were found to be significantly associated with two types of imagery established by an independent inquiry into thinking habits. First, a regular respiratory type (regular in rhythm and amplitude), persisting whilst tasks were performed, was found in subjects with predominantly visual imagery. Secondly, an irregular respiratory type was found with subjects having mainly auditory imagery. When a problem which appeared to need purely visual imagery was attempted, the regular type persisted, the irregular type became regular. The tendency to vocalization, plus laryngeal movements, was held to be responsible for the irregularity of breathing. These findings were substantiated by Paterson (6) and by Wittkower (10).

(ii) *Alpha-rhythms and imagery*

Golla, Hutton & Grey Walter (3) established another physiological concomitant of imaging in 1943, viz. the electrical activity of the posterior areas of the brain, recorded by means of the electro-encephalogram (EEG). The best known rhythms from these areas are the alpha-rhythms, with a rhythmic fluctuation in voltage of 8–13 cyc./sec. On the basis of experimental findings, they suggested the following fundamental classification of alpha records, also adopted for analysing records obtained in the present experiment. Records fall into three main groups:

(*a*) *M* (minus) type. Records with alpha-rhythms below 10μV. maximum in amplitude; the effect of opening and closing the eyes, and the effect of mental activity with eyes closed, being undetectable.

(*b*) *R* (responsive) type. Records with alpha-rhythms within the normal range of amplitude (10–50μV.) with eyes closed; blocking entirely, or much attenuated with eyes opened; and diminished if mental activity proceeds while the subject's eyes are closed.

(*c*) *P* (persistent) type. Records with alpha-rhythms of normal amplitude, persisting during mental activity relatively unchanged or in 'bursts' and unaffected by the eyes being opened or closed.

The 'mental activity' referred to above was usually a simple sum in mental arithmetic. When 'blocking' of alpha-rhythms occurred, the subject very often reported afterwards

that he had been visualizing the numbers. An example of a record obtained in this way is shown in Fig. 1.

A general inquiry into thinking habits, and the independent performance of seven mental tasks, showed that the *M* types used mainly visual imagery in thinking, the *P* types chiefly auditory-kinaesthetic imagery; and with the *R*'s either type of imagery might predominate. A preliminary and independent survey of respiration showed that the *M* types tended to breathe regularly, the *P* types irregularly.

What follows is an attempt to implement a proposal made in 1943 for 'an intensive study of the relations between various forms of imagery and the electrical activity of the cortex', together with a concurrent examination of breathing. Special efforts were made, first, to base an assessment of imagery type on the performance of mental tasks whilst physiological recordings were made; and secondly, to register concurrently, on the *same* record, alpha-rhythms (where present), and respiratory movements.

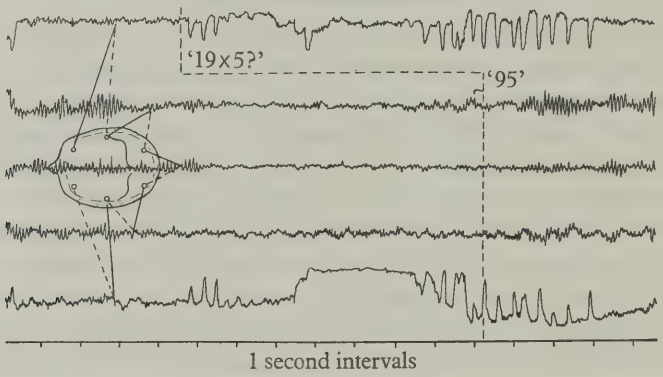

Fig. 1. The EEG of a subject with a *responsive* alpha-rhythm. The record shows a characteristic blocking of the alpha-rhythm when the sum is given. The rhythm returns to its previous amplitude once an answer is found. The diagrammatic head shows the positions of the electrodes: each tracing is of the difference in potential between two electrodes, shown by pairs of lines connecting the electrode positions to the records. Vertical scale: 0·5 in. = about 50 μV.

III. The subjects

There were two groups of subjects. The first consisted of 75 out-patients of the Burden Neurological Institute, age range 8–59 (S.D. = 15·7), of whom 63 were diagnosed as idiopathic epileptics. In the second group were 75 normal subjects, age range 18–37 with an S.D. of 5·5. Sixty-three of these were University students. It was at first intended to use the normal subjects as controls, but as no significant differences were found between the groups either in the verbal reports, or in the EEG alpha and respiration records, it was decided to combine them into a single group of 150 subjects for statistical purposes.

IV. Methods

(i) *Assessment of imagery*

'Objective' tests of the questionnaire type were rejected for the reasons already given. Only one such test, the *cube problem*, was included. Simple task situations were employed to determine the kinds of image most often experienced by subjects. After performance the subject was asked for as full an account as he could readily give of his thinking and imaging experiences whilst he was performing the tasks. These remarks were written

verbatim on the EEG paper as the subject was speaking. Provided the term 'visual image' was taken as equivalent to phrases like 'picturing to myself', or 'imagining myself seeing', and the term 'verbal-motor image' was regarded as equivalent to phrases like 'talking', or 'saying to myself', then the task of retrospection was no more difficult for patients than for the University students (of whom only two were studying psychology).

The experimenter took care not to influence subjects when observing the behaviour of the tracings. These might well have led him to 'expect' certain answers and to frame his inquiries accordingly. The interview was therefore carefully standardized.

(ii) *Recording respiration*

As *concurrent* recordings were needed, the methods of recording respiration used by Golla & Antonovitch (2) and by Golla *et al.* (3) were not used, as both yielded *separate* respiration records. In the first study, displacement of air caused by the subject's breathing as he sat in an air-tight box was recorded with regular or irregular excursions of an ink tracing on a rectangular plethysmograph. This was of the familiar float pattern, with a capacity of about four litres. In the second experiment, a rubber sac was adjusted to the thorax, recording through a Marey capsule.

In the present study, where regularity and depth are the only features of respiration considered, a thermocouple was used. It converted temperature variations in front of the nostrils into electrical variations, recorded in channel 1 of the EEG amplifier. Electrical activity of the parieto-occipital areas of the cortex was recorded in channel 2. Under these conditions any changes in breathing rhythms could be directly compared with the behaviour of any alpha activity.

It was also essential for the subject not to know the real purpose of the apparatus, so it was fixed to a spectacle frame. A very light, robust apparatus, much less cumbersome than the coffin-like arrangement of the plethysmograph, it afflicted the subject no more than the wearing of any other pair of spectacles without lenses. None of the subjects guessed what it was for. Had they done so, their breathing might have been affected in various unpredictable ways.

To decide whether breathing was irregular or not, each breath had to be measured. The distance from one inspiration (upstroke in the figures) to the next, was measured in centimetres on the record and converted directly into seconds throughout. For this purpose, the paper speed (rate = 1 cm./sec.) was an asset: each breath measured in centimetres and millimetres was plotted directly as a point in the appropriate vertical column in seconds and tenths of a second on a 'Breath Time Chart'.

(iii) *Recording alpha-rhythms*

A preliminary examination on a 6-channel EEG, with automatic analyser, was given to all subjects. This included, for patients, recordings of (1) spontaneous rhythms from all areas of the brain with eyes opened and closed, (2) rhythms during hyperpnoea performed for 3 min., and (3) rhythms with photic stimulation, eyes opened and closed. For normal subjects this procedure was the same except that (2) was omitted.

Patients who showed marked abnormalities in the parieto-occipital regions did not act as subjects. The presence of recognized abnormalities in recordings (2) and (3) had no effect on the experimental investigation of alpha-rhythms. About one-third of the total intake of all out-patients for a period of 4 months acted as subjects. This first examination was valuable also because it familiarized and adjusted most subjects to EEG recordings.

The experiment proper began with the second examination made with a 2-channel portable EEG. The subject sat in a chair. Two scalp electrodes were placed under a cap over the parieto-occipital areas on the right or left side according to the region where the alpha-rhythms were of greater amplitude. In practice, it made little difference from which side potentials were recorded for the presence and blocking of alpha-rhythms. Frequency changes in alpha-rhythms with eyes opened and then closed, occasionally found in *P* types, showed no effect on task performances.

V. Procedure

First, the routine EEG examination was given to all subjects. The investigation of imagery on a portable EEG took place next in a separate sound-proof room. A minute or two was allowed for each subject to settle down once the recording began. The next 5 min. were devoted to the discovery of alpha type and habitual breathing while the subject was at rest. This is known as the 'initial breathing period'. In several cases this period lasted for a quarter of an hour or more when the experimenter was out of the room and no marked changes occurred. At the end of the experiment, a further period of relaxation was given, and respiratory tracings then were very similar to those obtained during the initial period. Sometimes the *rate* of breathing decreased, but this did not affect the overall regularity or irregularity.

Before tasks, subjects closed their eyes. The following standard inquiries were then made, with a minute interval between tasks:

(1) *Sum.* 'Will you multiply 19 times 5?' If the answer was given within 5 sec., a more difficult sum was given.

(2) *Story.* 'Will you think over the story of Noah's Ark?' (or 'Cinderella' if the former were not known).

(3) *The cube problem.* 'Think of a cube ('square brick' for children), which is painted red on the outside, but is plain inside. Cut it in halves; and now cut it in halves again. How many sides of all the pieces will be painted red, and how many will be unpainted?'

(4) *Prayer.* 'Will you think over the Lord's Prayer?'

(5) *Anthem.* 'Will you think over the National Anthem?'

(6) *Argument.* 'Will you think over points for and against the Labour Party, and points for and against the Conservative Party?' Children were requested: 'Think about what you would like to do very much when you grow up, and what you would hate to do.'

On completing each task the subject was asked: 'In what way did you think about that? What was going on in your mind when you were thinking of, for example, the National Anthem?' Replies were written exactly as given on the records, and later carefully classified.

In assessing records, two factors which had nothing to do with imagery but occasionally caused irregular breathing and masked alpha activity, were discounted from the results. The first factor was a spasmodic restless anxiety or fidgeting, recorded on the EEG as muscle action potentials in the form of a fast 'spikey' activity. Of course, the preliminary recording helped to reduce the chances that this reaction would occur, but it appeared for brief intervals with a number of subjects. With five subjects it persisted at length; they were considered unsuitable for experiment and were replaced by others. Secondly, at the

outset of the more difficult tasks, like the 'Argument', a certain tension occurred in some ten of the subjects: respiration records showed that breath was held or inhibited for periods up to 14 sec., and alpha-rhythms were blocked. The respiration effect was shown in similar recordings by Golla & Antonovitch in 1929 (2). They found either shallow and quickened breathing or inhibition in the inspiratory phase. Similar phenomena were described by Golla in his Croonian Lectures (5), showing that they bore a relation to the increased tonus of skeletal muscles, demonstrated by myographic methods during intellectual effort. This was later confirmed by Golla & Antonovitch (4) by observations on the tendon reflexes.

The occurrence of both effects, initial tension and restlessness, is the justification for ignoring the first part of such records in relation to the type of imagery used. What happens is that the individual is summoning all his resources to deal with an emergency situation; there is a mild intellectual crisis. Once the material is mustered, the tension disappears quite quickly. Images then emerge and are utilized.

VI. Results

(i) *Classification of imagery*

Counts were taken of the mention of images, or their colloquial equivalents (as described), in whatever modality they were said to occur. Some subjects claimed to be aware during the task periods mainly of visual images, others mainly of verbal-motor images. The former were classed as 'visualists' and the latter as 'verbalists'.

A more careful examination of reports revealed two main categories. These were named 'sensory' and 'kinetic'. In the sensory group visual imagery occurred much more often than any other imagery type. Auditory images were also included, where subjects reported the 'hearing' of sounds, not, however, self-produced, but originating in some way 'outside' themselves. With music, the subject imagined hearing music as played by an orchestra or as sung by a choir, but did not imagine himself to be either singing or humming.

A small and very interesting subgroup were aware of 'self-produced' verbal images, but were at pains to describe that they were non-kinetic, or non-motor. None of these were psychologists. Four of them claimed to be using their *own* voices, but in a *non-articulate* fashion. One described his imaging as: 'verbalizing but not voicing', and another as 'speaking in a non-motor fashion'. Three out of the five normal subjects with this type of imagery had 'abnormal' findings in the first EEG examination. In all cases these were 'wave-and-spike' discharges evoked by photic stimulation at 18 to 25 flashes per second. This finding is puzzling, because patients showing a similar abnormality in their records did not use this persistent verbal-sensory imagery; nor were there any other similarities between these 'normal' subjects and such patients. Three other normal subjects had 'abnormal' EEG's, but with no peculiarities in imagery.

In the kinetic group, motor images of words occurred; the individual imagined himself to be uttering words or perhaps singing or humming them. Sometimes this became quite overt, especially with children. Subjects might or might not be aware of laryngeal or mouth or lip movements.

Many subjects experienced both sensory and kinetic images during tasks, usually with a predominance of one type. At times the type of imagery varied according to the nature of the task.

(ii) *Respiration types and imagery types*

Extracts from the breath time charts for three individuals are shown in Fig. 2. Breaths recorded when subjects were resting, and when they were occupied with tasks are distinguished on the charts. For the sake of clarity breaths recorded *after* tasks were completed are omitted; they were much the same as those recorded during the initial breathing period.

Regularity and irregularity of breathing for each subject was determined by the measurement of what is known as the 'breathing span'. This is the difference, in seconds, between the longest and the shortest breath times. Spans were measured for the initial resting period, for all tasks, and for a period when tasks were completed. For example, in Fig. 2, with subject 1 when resting, the difference or span between the shortest breath (2·3 sec.) and the longest (6·6 sec.), is 4·3 sec. He is an irregular breather. So also is subject 2 (span, 3·9 sec.). But with subject 3 the span is only 0·5 sec., and he is a regular breather. It can also be seen from Fig. 2 that the shorter the span when resting, the greater is the regularity when doing tasks and vice versa. This is the general rule.

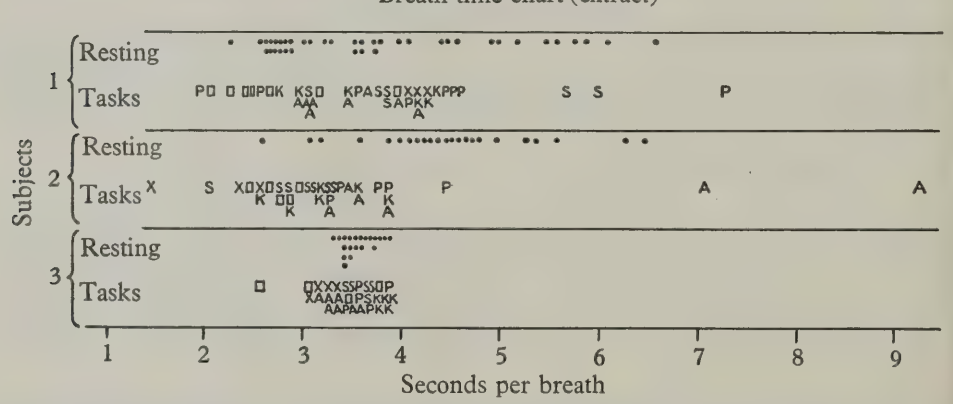

Fig. 2. Breathing records of three subjects (see text). Subject (1), irregular whilst resting and during tasks; subject (2), irregular whilst resting and during tasks; subject (3), regular whilst resting and during tasks. Breaths recorded during resting are shown: •, in the upper half of each chart; breaths during tasks, in the lower half. Breaths during the *sum* task are shown X; *story* task, S; *cube* problem, □; *prayer* task, P; *anthem* task, K; and *argument* task, A.

Occasionally, with longer initial resting spans (i.e. more than 1·0 sec. and less than 3·0 sec.) considerable variations in span occurred for different tasks. A subject would breathe very regularly (with a very short span) during, say, the cube problem, but show very irregular respiration (with a very long span) during, for example, the 'Anthem' task. In such a case the span of the initial habitual breathing period tended to be midway between the very short span and the very long one. But, usually, from a measurement of the initial breathing spans, it is easy to predict the kind of span to be expected, in accordance with the general rule given. Indeed, in many cases, measurement is hardly necessary, and a reliable prediction can be made by merely glancing at the continuous recordings of the respiration curves in the early parts of the EEG records themselves.

Of the subjects examined, 76 or 50·7% were regular breathers, and 74 or 49·3% were irregular. These figures correspond quite closely to those given by Golla (2), 54·5% regular and 45·5% irregular; and by Paterson (6), 49·2% regular and 50·8% irregular.

Of the 80 visualists, 66 or 82·5% were habitually regular breathers; only 14 of the visualists, or 17·5%, breathed irregularly. On the other hand, of the 70 verbalists, 60 or about 86% breathed irregularly all the time; and only 10 verbalists, or about 14%, were irregular breathers. These results are displayed in Fig. 3, which shows at a glance the general correlation between subjective assessments and breathing spans during resting.

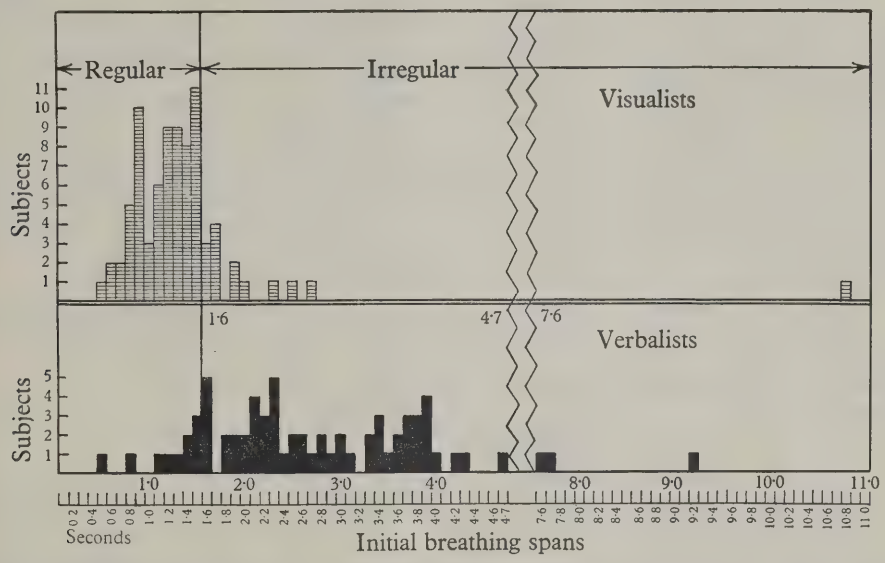

Fig. 3. Regularity and irregularity of breathing for all subjects during resting period. Horizontal base-line shows the breathing spans (see text) from 0·5 to 10·8 sec., in steps of 0·1 sec. Numbers of subjects for each span are plotted vertically.

During task-periods, the habitual regularity or irregularity of both groups tended to increase. Primary visual or verbal-motor imagery respectively, was reported. This supports the findings of Fox (1), that images occur in thinking chiefly when a difficulty or problem arises. However, provided that the subject is not actually asleep, he will presumably be thinking about something, enough at any rate to characterize his respiratory type.

The figures given above of associations between breathing when resting and the imagery type, were submitted to the χ^2 test, although this was barely necessary in view of the obvious weighting in the proportions given. $\chi^2 = 68{\cdot}76$; the probability that the associations named were due to chance was less than 0·001 against. Similarly, reliable statistical results were obtained with all tasks. They are given in Table 1.

It will be noticed in this table, and in Table 2, that tasks 2 and 6, the *story* and *argument*, are combined in association (2), and tasks 4 and 5, the *prayer* and *anthem*, in association (3). Tasks 2 and 6 appeared to involve one type of thinking, namely, 'problem' thinking, but tasks 4 and 5 entailed, for most subjects, merely rote recall. In a sense, the contrast between the two types of thinking resembles the differences in subject's reactions to the material used by Fox (1), i.e. propositions entertained with greater or lesser difficulty, and those propositions to which 'ready assent' was given.

Even where the *prayer* or *anthem* were not remembered with ease, the difficulty experienced in recall was not the same as the difficulty involved in thinking out a

Table 1. *Associations between imagery and respiration for tasks*

Tasks	Imagery types	Imagery	Respiration: Regular	Respiration: Irregular	Total	χ^2	P
(1) Sum	Visualists	Sensory	52 (51 visual)	2	78	40·62	<0·001
		Kinetic	7	17			
	Verbalists	Sensory	30 (27 visual)	4	71	36·84	<0·001
		Kinetic	6	31			
(2) Story and argument	Visualists	Sensory	112 (98 visual)	5	156	75·70	<0·001
		Kinetic	12	27			
	Verbalists	Sensory	33 (26 visual)	10	134	48·27	<0·001
		Kinetic	14	77			
(3) Prayer and anthem	Visualists	Sensory	87 (61 visual)	1	156	91·35	<0·001
		Kinetic	18	50			
	Verbalists	Sensory	27 (8 visual)	2	143	61·89	<0·001
		Kinetic	19	95			
(4) Cube problem	Visualists	Sensory	72 (all visual)	5			
		Kinetic	0	3	82	—	—
		No imagery	1	1			
	Verbalists	Sensory	44 (all visual)	6			
		Kinetic	0	13	68	—	—
		No imagery	3	2			

comparatively novel situation. In the first case, inhibition of recall tended to occur first as a kind of mental blankness. For example, the subject doing the *prayer* task, in Fig. 6, said: 'I didn't get beyond the first sentence. I was puzzled after that, and didn't "say" any more.'

On the other hand, apart from an initial tension occurring in a few cases, any difficulty experienced in the *story* or *argument* tasks favoured as a rule the emergence of images belonging to the subject's habitual mode of imagery. Yet difficulties of the kind mentioned in tasks 4 and 5 were encountered more by verbalists than by visualists; failure in recall was less frequent with those who could envisage scenes than with those who relied solely on verbal-motor images. Hence, these tasks also afforded certain clues to the habitual imagery mode.

During the *story* task, owing to the shorter period occupied in thinking, a shorter span of 1·1 sec. marked the transition between regular and irregular breathing instead of the 1·6 span in the initial resting period (Fig. 3).

(iii) *Alpha types and Imagery types*

Typical records are shown in Figs. 4–6. It was found that the great majority both of visualists (56) and of verbalists (44) had records of the '*R*' type (Fig. 4), responsive by suppression of the alpha-rhythms. There remained only 18 visualists and only 3 verbalists who were *M* type (Fig. 5) with no alpha-rhythms; and 24 verbalists with only 7 visualists who were *P* type (Fig. 6) with alpha-rhythms persistent. Imagery with these last two groups tended to be of an extreme nature; exclusively visual with the *M*'s and almost constantly verbal-motor with the *P*'s. The *R* group was the largest, and was intermediate

between the other two, showing mixed imagery, but with a definite predominance of visual or verbal-motor images in individual subjects. The fact that the two distributions of alpha type and imagery type significantly overlap each other is shown by the χ^2 test. $\chi^2 = 20{\cdot}25$, $P = < 0{\cdot}001$.

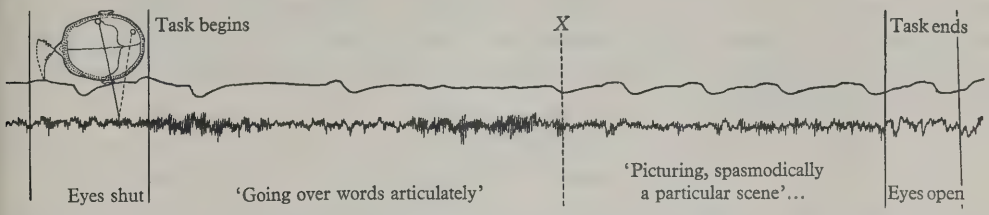

Fig. 4. Alpha type: *R*, a 'verbalist'. During 'initial period' respiration irregular (span, 3·5 sec.). *Argument* task period illustrated. Respiration irregular up to '*X*', then becomes more regular (span, 0·6 sec.). Note corresponding behaviour of alpha-rhythms.

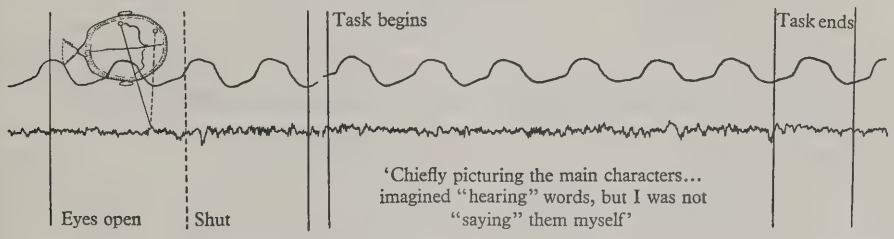

Fig. 5. Alpha type: *M*, a 'visualist'. Respiration very regular during the 'initial period' (span, 0·5 sec.), and throughout all tasks. The *story* task period is illustrated, and respiration remains completely regular (span, 0·2 sec.).

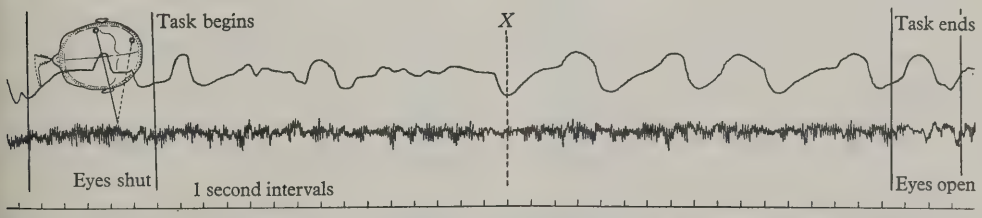

Fig. 6. Alpha type: *P*, a 'verbalist'. Respiration very irregular during the 'initial period' (span, 4·3 sec.). *Prayer* task period illustrated. *Note.* Respiration irregular up to '*X*' (span, 4·4 sec.), then becomes more regular (span, 1·0 sec.). Alpha-rhythms persist throughout. Subject evidently verbalizing up to '*X*', but not afterwards. No visual imagery. Report at end of task was: 'I didn't get beyond the first sentence, (claimed to be articulated). I was puzzled after that, and didn't "say" any more.'

A cross-correlation between alpha types and respiratory types in Table 2 shows 12 *M*'s with regular respiration, 7 with irregular; 23 *P* types show regular respiration, and 8 irregular. This result is just significant ($P = 0{\cdot}01$). This association becomes much more significant statistically when the testing situation is substituted for the resting period. The chances are about equal (56:44), that a subject in the *R* category will be an irregular or regular breather. What this involves during tasks is clear in the following figures. Of the 56 habitually regular breathers, 50 were visualists with regular respiration during tasks; 6 were verbalists with either regular or irregular breathing during tasks. Of the 44 habitually irregular breathers, 37 were verbalists with mainly irregular breathing during tasks, and 7 were visualists with both regular and irregular respiration during tasks.

Task periods were examined in detail to see how often the alpha-rhythms blocked when the subject was visualizing, and how often they persisted when he was verbalizing. The result was quite conclusive. Out of 833 instances of imagery reports and corresponding alpha activity, 517 or 62·8% were alpha responses (or blockings of alpha-rhythms), with visual imagery, and only 67 or 8% with verbal-motor imagery; 200 or 23·3% were alpha persistences with verbal-motor imagery, and only 49 or 5·9% with visual imagery. Detailed statistical analysis of each task, given in Table 2, simply confirmed the results of the main overall analysis.

Table 2. *Association between alpha types and respiratory types*

	Alpha types		
	M	*R*	*P*
Respiratory types			
Regular	12	56	8
Irregular	7	44	23

Total = 150; $\chi^2 = 9{\cdot}988$; $P = 0{\cdot}01$.

Table 3. *Associations between imagery and alpha-rhythms for tasks*

Tasks	Imagery types	Imagery	Alpha-rhythms: Responses	Alpha-rhythms: Persistences	Total	χ^2	*P*
(1) Sum	Visualists	Visual	39	7	62	10·04	0·01
		Non-visual	7	9			
	Verbalists	Visual	35	2	64	27·27	<0·001
		Non-visual	9	18			
(2) Story and argument	Visualists	Visual	84	7	114	39·01	<0·001
		Non-visual	8	15			
	Verbalists	Visual	50	9	124	51·88	<0·001
		Non-visual	13	52			
(3) Prayer and anthem	Visualists	Visual	68	5	130	56·71	<0·001
		Non-visual	17	40			
	Verbalists	Visual	36	6	130	52·07	<0·001
		Non-visual	17	71			
(4) Cube problem	Visualists	Visual	59	2	64	—	—
		Non-visual	0	1			
		No imagery	0	2			
	Verbalists	Visual	55	3	66	—	—
		Non-visual	2	1			
		No imagery	2	3			

VII. Discussion

This inquiry showed that subjects fall into two main categories of imagery, visual and verbal-motor. The extreme alpha types correlate with extreme types of imagery, and the *R* type with moderate or mixed imagery, yet with a clearly recognizable predominance of visual or verbal-motor images during tasks. The visualists tend always to breathe regularly, and the alpha-rhythms, where present, block whenever they are busy with mental tasks; visual images come more readily to them than verbal-motor ones. The verbalists tend always to breathe irregularly, and their alpha-rhythms tend to persist whether they are thinking out problems or not. This presentation of results is primarily a statistical survey, and its aim is to indicate the presence of significant trends of behaviour, psychological and physiological.

Considering psycho-physiological correspondences in more detail, the association during tasks between alpha responsiveness and respiratory movements, whilst clearly significant in the *generality* of correlative changes, demands a more elaborate study of fewer subjects with tasks recorded at greater length. Four main obstructions limited the study of this association in this experiment.

First, a score of 'alpha responsiveness' in the results included many rapid and subtle variations which could not be scored at the same time as the accompanying respiratory changes. Very short 'bursts' of alpha activity occurred quite often, lasting for 1 or 2 sec., and correlative breathing changes were not registered owing to the *rate* at which respiration was recorded.

Secondly, with persistent alpha activity, concurrent respiratory movements might be regular *or* irregular, according as imagery was sensory or kinetic, even where it was non-visual. The relation of non-visual sensory imagery to thinking needs more thorough study of the few subjects showing this unusual mode of imagery. The abnormal EEG records in a few cases have already been noted.

Thirdly, the complexity of the *M* group is shown in the analysis of all tasks for the twenty *M* types. Eighty-five instances show regular respiration, thirty-five show irregular respiration. With *M* types regular respiration would *always* be expected, but the question as to whether alpha-rhythms should emerge when mixed imagery is reported, needs further study. A test might be devised giving a prolonged and powerful auditory stimulus to an *M* type, completely occupying his attention, to see whether or not alpha-rhythms emerge under such conditions. At present, with a true *M* type, it appears that with verbalization alpha-rhythms *never* emerge, whatever imagery is spontaneously reported, and despite changes in respiratory rhythm.

Fourthly, even where there were correlative changes in respiration and alpha responses, retrospective references to time, by untrained subjects, were usually too unreliable to make it certain that verbalizing, for example, occurred at a very specific determinable point during tasks. Certain quite detailed correlative changes *did* occur which could be linked up roughly with imagery reports; examples are shown in Figs. 4–6. This was sufficient for statistical purposes. Some self-operated signal is needed (e.g. pressing a lever), in studies made at length with trained subjects indicating introspective changes that occur during the *tasks themselves*, and not retrospectively. The problem is how to use current signals so as not to interfere excessively with the pattern of thinking.

In the experiment, no special study was made of intelligence nor of the subject's *ability* to deal with individual tasks. It is arguable that intellectual skill resides only in the manipulation of images, however restricted we may be in the type of images at our disposal. The more an individual can adapt his image-models to suit his problem, the greater may be his efficiency in dealing with a variety of mental tasks. But if we can utilize visual or verbal-motor images with about the same ease, then clearly we are better fitted to deal with more problems, whether they demand mainly visual or verbal aids.

Of all the alpha types, persons belonging to the *R* group are the most favoured in so far as they can readily adapt their models for different types of mental problems. While not obsessed with private pictures, they can evoke satisfactory visual patterns when necessary and can combine data from the various sense organs more readily than can either the *M* or *P* types. In the cube problem, most moderate verbalists (chiefly *R* types),

were able to picture the cube because this was the easiest way of solving the problem. Sixteen subjects, mostly *P* types, found considerable difficulty in visualizing the cube and tried to rely exclusively on counting or calculating. Paterson (6) found that he had a similar proportion of habitual irregular breathers amongst his subjects. His argument fits well into the present context: 'the fact that 13 subjects (out of 144), used irregular rhythm throughout, seems to indicate that they were persons who had difficulty in using visual imagery exclusively at any one time, even where the problem seemed to require the visual type of thinking.'

The subject, a *P* type, whose record is given in Fig. 6, had his alpha type been different might have recollected the words of the remainder of the prayer by visual means, instead of trying to recall them by purely verbal methods. But visual imagery was absent from his reports, and presumably from his mental life as a whole. Similarly, the *argument* demands some use of words. The report of a subject of the *M* type, an extreme visualist who never verbalized, shows just how bizarre exclusive visualizing can be with this type of problem:

A map of England with the centre at London, with black lines radiating symbolizing nationalization. The Conservatives are represented by spots without connecting lines, with the words 'free enterprise' written. I was neither hearing nor saying anything.

Such evidence, together with the further analysis made of the varying successes and failures with simple mental arithmetic problems achieved by different imagery types, suggests that imagery differences with their special physiological characteristics may be of fundamental importance in the basic structure of personality. The argument is strengthened by the fact that statistical and experimental findings indicate that the alpha types, chiefly the *M*'s and *P*'s, as well as the alpha frequencies, are inborn and probably inherited. It is very likely, therefore, that a detailed study of the relation of imagery types to personality differences would be rewarding.

VIII. Summary

1. A preliminary review indicated:
   (*a*) That as yet the best method of discovering the subject's habitual modes of imaging is to give him mental tasks and to classify his reports directly after the performance.
   (*b*) That perceiving and imaging belong to a psychological and physiological continuum; i.e. that seeing and visual imagery, and speech and verbal-motor imagery are not different in kind, and similar mechanisms may account for seeing and visualizing, and for speaking and 'verbalizing', respectively.

2. Hence, a study was made of:
   (*a*) The electrical activity of the visual association areas of the cortex, recorded by means of the EEG.
   (*b*) Respiratory movements, recorded by means of the thermocouple. The two factors were examined concurrently. The results were related to the reports of imagery.

3. It was found:
   (*a*) That of the two main imagery-types, visualists breathed regularly and showed frequent blocking of alpha-rhythms, both of these physiological characteristics being associated with actual visualizing. Verbalists, on the other hand, breathed

irregularly, with alpha persistences, both characteristics being associated with verbal-motor imagery. As a rule, when verbalists attempted a problem requiring predominantly visual images, then physiological changes occurred characteristic of visualists.

(*b*) That amongst the alpha types, the *M*'s used mainly visual images in thinking, the *P*'s chiefly verbal-motor images; with the *R*'s there was mixed imagery, usually with a predominance of visual or verbal-motor images.

(*c*) That verbal-sensory imagery is unusual if not abnormal. It was reported habitually by a small minority of subjects only.

4. The discussion indicates the complexities involved in this type of psycho-physiological recording, and the need for further refinements in technique. It also suggests that imagery studies are important for the examination of personality differences.

I should like to thank Prof. F. L. Golla for encouraging me to start on this investigation, and for permitting me to publish the results. I am much indebted to Dr W. Grey Walter for his generous advice and constructive criticism whilst work was in progress, to Mr A. L. Winter for kindly taking the preliminary EEG records, and to the students of Bristol University and others who so readily acted as subjects.

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PREPARATORY SET (EXPECTANCY)—AN EXPERIMENTAL RECONSIDERATION OF ITS 'CENTRAL' LOCUS

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I. *Introduction* (pp. 52–53). II. *The Experiment* (pp. 53–54): (i) *General hypothesis* (p. 53); (ii) *Subjec* (p. 53); (iii) Apparatus (pp. 53–54). III. *Experiment A* (p. 54): (i) *Method* (p. 54); (ii) *Results* (p. 54 IV. *Experiment B* (pp. 54–55): (i) *Method* (pp. 54–55); (ii) *Results* (p. 55). V. *Experiment C* (p. 55 (i) *Method* (p. 55); (ii) *Results* (p. 55). VI. *Discussion* (pp. 55–56). VII. *Summary and conclusic* (p. 56). *References* (p. 56).

I. Introduction

Problems in science are most often considered 'solved' when data are collected whic permit investigators confidently to accept or reject certain hypotheses. Sometime however, problems are periodically discarded either because they are intrusions fro 'meta-science' and depend, therefore, upon emotional bias for working solution (e.g. tl nomothetic-idiographic question, the mechanism-vitalism controversy) or because th are framed in a manner demanding dichotomous answers where the nature of the variabl and their interaction seem to preclude all-or-none categorization. (Is this behaviour d to heredity *or* environment?) Such issues usually keep recurring in different guises.

One such problem, popular a decade ago—that of the locus of set—was abandoned f lack of crucial evidence. The question, as phrased, asked whether a central facto independent of the contribution of peripheral mechanisms (sense organs and muscle could be demonstrated in the increased readiness to respond, termed 'set'.

Mowrer, Rayman and Bliss (4) have argued for such a central determinant, citing evidence the fact that when subjects, instructed to respond to either light or sound, expe a light, their reaction time is faster when a light is forthcoming than when a sound presented. Freeman (1), with a more peripheralistic bias, has rightly rejected the Mow study as ambiguous, because sense-organ adjustment could also conceivably be differe for the two conditions of expectancy.

Mowrer (3) rejoined with data derived from a further experiment in which a tacti stimulus was substituted for the light. The rationale for using the skin receptors was th since they have no known mechanism for preparatory adjustment to stimuli, no differen should be found between reaction time, sound-expected, and reaction time, tacti vibrator-expected, unless some central mechanism was operating. Even here, of cour as Munn points out, Mowrer has not furnished a clear-cut demonstration of the cent locus of preparatory set.

'One could argue that the subject's ear was set for sound and that immediately up failure of the sound to appear, its relaxation retarded the motor response throu interference of related neural activities with those involved in making the finger reacti Such an interpretation may be far-fetched, especially since the difference in react time under the two conditions was sometimes as small as one-twentieth of a seco Nevertheless, we cannot completely ignore such a possibility.' ((5), p. 392.)

In view of the fact that functional independence of the two regions, central a peripheral, has not been empirically established, the present writers do not purport o

provide a final answer to the question of locus of preparatory set. It is, however, believed that one may create a situation to demonstrate more adequately the possibility of the decisive operation of a central mechanism such as Mowrer favours.

Such a demonstration would entail two major controls. In the first place, the stimuli selected should be such that trial-to-trial differential in the pre-stimulation sense-organ adjustment will not occur. Secondly, the response-pattern should be identical throughout the series.

This latter specification, identity of response, has already been met in the Mowrer studies, a single response (releasing a key) being called for in all instances. A comparable control is, of course, incorporated into the present study. The former control, ensuring constant prestimulation sense-organ adjustment, unsatisfactorily maintained in the Mowrer design, may be achieved by conducting the experiment in a single sense modality with stimuli which make relatively the same physiological demands on the individual.

II. THE EXPERIMENT

The present study consists of three experiments, the second evolving from difficulties arising in interpreting the results of the first and the third being derived similarly from the second.

Differences in reaction time yield a convenient index for inferring the operation of preparatory sets. This general approach is employed here; its advantages have been aptly elaborated by Mowrer (2), and need not be further discussed.

In an effort to realize the sense-organ control previously specified, the experimenters used small achromatic geometric figures, equal in area and exposed on as nearly as possible the same regions of the retinae. The reaction mechanism, instead of finger movement, was verbal response.

(i) *General hypothesis*

If an individual is instructed to respond as quickly as possible to stimuli which require the same pre-exposure end-organ adjustment but about which he has different amounts of preliminary information, latency of response will be shorter in those instances in which the preliminary information is greater. Assuming equalization of trial-to-trial peripheral-sensory and peripheral-motor conditions, differences in response latency with differences in amount of preliminary information may be interpreted as indicating differential central adjustment.

(ii) *Subjects*

Thirty-five students from Emory University, naïve as to the specific purpose of the experiment, served as subjects; ten in Exp. A; ten in Exp. B; and fifteen in Exp. C. Both control and experimental observations were obtained from each subject.

(iii) *Apparatus*

Stimulus exposure was effected by a Tridimensional Vivid projection-tachistoscope, placed 75 cm. behind a 25×25 cm. ground-glass screen framed by a 75×60 cm. masonite mask painted flat white. The subject was seated in a dimly lighted room approximately 1·75 m. directly in front of the screen. One microphone of a Lafayette electronic voice reaction-time apparatus with cumulative chronoscope was attached to the projector. This microphone picked up the click of the shutter made in figure exposure and activated the

timer. The subject's verbal response to the figure, relayed by a throat microphone stopped the timer. Three slides, each containing a 12·25 sq.cm. opaque form (triangle circle, or square) constituted the stimulus objects.

III. Experiment A

(i) *Method*

Each subject was given the following instructions:

'This is a study in reaction time. Under certain circumstances it is important tha one respond both quickly and accurately. We want to see how quickly and accuratel you can respond to simple geometric figures—triangle, circle, and square—flashed on th screen before you. As soon as you recognize the form you are to report what it is. O some trials we will simply say "Ready" to indicate the forthcoming presentation; o others we will give you some information about what to expect. In either case you are t respond quickly and accurately, reporting what you see as soon as you see it.'

Each subject then received singly and in random order twenty-four one-fiftieth-se second exposures of each stimulus figure. Preceding one-half of the trials for each form he was given correct information concerning what the figure to be presented would be for the other half he was given no information. In cases where a subject incorrectl identified a form, the trial was repeated at a later time in the series.

(ii) *Results*

As Table 1, line 1, indicates, the results lend support to the hypothesis. The adequac of the peripheral-motor control may, however, be questioned. Because three differer

Table 1. *Mean response latencies with and without pre-exposure identification of the stimulu figure for experiments requiring various degrees of response precision (increasing fro Exp. A to Exp. C)*

		Response latency (in $\frac{1}{100}$ sec.)					
	Exp.	With pre-exposure identification	Without pre-exposure identification	Diff.	t	D.F.	P
(1)	A	50	57	7	6·43	9	<0·01
(2)	B	41	46	5	9·48	9	<0·01
(3)	C	42	44	2	3·43	14	<0·01

vocalizations are required—'triangle', 'circle', 'square'—one may argue that reactio time differences are not proof of the operation of a central mechanism but may attributable to differential pre-response adjustments in the vocal musculature. Exp. was consequently devised to meet this objection.

The repetition of trials in which the subject responded incorrectly proved unnecessa since only six false responses occurred out of the possible 720, these being spread ov five subjects.

IV. Experiment B

(i) *Method*

This experiment is similar to the preceding except that in the instructions the subje was asked to respond '*a* triangle', '*a* circle', or '*a* square', thus making identical t

crucial chronometer-stopping response. The throat muscles would be set to say '*a*' in all cases.

(ii) *Results*

Inspection of Table 1 again shows the hypothesis verified (line 2). Argument may again be interposed that although the crucial response, '*a*', is superficially and linguistically identical in all cases, psychologically it may be fused with the word following it. The peripheral adjustment underlying these various psychomotor response-units ('*a*-triangle', '*a*-circle', '*a*-square') might then be different. Exp. C represents, therefore, an attempt for more complete peripheral-motor control.

V. EXPERIMENT C

(i) *Method*

Exp. C differed from the preceding in several ways. The number of stimulus figures was, for the purposes of simplicity, reduced to two (square and circle) with only twenty presentations of each. In view of this shortened series, six practice trials were given before the test series. The subject's response to all figures was simply 'Recognize', the instructions being modified accordingly. The difference in verbal response removed, attempt was made to monitor the subject's figure-discrimination by introducing eight 'check' trials. The subjects consequently received the following *additional* instructions:

'In order to make sure that you are responding to the figures, a number of "check" trials will be interspersed randomly throughout the series. These checks consist of a simple flash of light; do not make any verbal response at all to these flashes.'

(ii) *Results*

Reference to Table 1, line 3, shows that these data also corroborate the hypothesis. The adequacy of the check trials is indicated by the small number of errors in response to them. The total of 120 check trials (eight per subject) yielded only two verbal responses, these not being obtained from the same subject.

VI. DISCUSSION

Table 1 reveals agreement among the results of all three experiments and provides confirmation for the thesis presented in the introductory remarks. That is, with constant peripheral adjustment, experimentally induced variations in central conditions (amount of pre-trial information) produce statistically significant differences in response latencies. The practical import of differences of such a minute order as those in Exp. C (one-fiftieth of a second) may, however, be questioned.

The presence of certain other differences within the table warrants comment. First, there is the generally lowered response latencies in Exps. B and C as compared to Exp. A. These, it seems reasonable to assume, may be attributed to the increased uniformity of the crucial response. In Exp. A, the subject must attach a verbal label to his perception of the presented figure before he can signal his recognition of it. In Exps. B and C, signalling may be accomplished simply by saying '*a*' in the one case, 'Recognize' in the other. Secondly, there is the smaller mean difference in response latency for trials with and without preliminary information in Exp. C (0·02 sec.) when compared to Exps. A and B (combined = 0·06 sec.). (Diff. = 0·04, $t = 4{\cdot}06$, D.F. = 33, $P < 0{\cdot}01$.) At least two possible

explanations for this reduced difference may be suggested: (1) the reduction in number of stimulus figures from three to two in Exp. C increases the subject's chances of correct anticipation on the no-information trials, this being reflected in shorter latencies for these trials; (2) the differential contribution of the peripheral-response-mechanisms to reaction efficiency is eliminated in Exp. C.

The pains taken to meet the demands of peripheral-motor control prompts a reconsideration of the present attempts at sense-organ control. While one might assume that the design (presenting each type of figure under both conditions of *information* and *no-information*) effectively eliminated type-of-figure as a determinant of the crucial difference in response latency, should an interaction between type of figure and amount of information be demonstrated, the present technique of receptor control would be inadequate. Accordingly, a threefold classification analysis of variance was applied to the data of Exp. C.

Examination of the summary of this analysis, Table 2, indicates not only no significant

Table 2. *Summary of triple classification analysis of variance of response latencies in Exp. C*

Source	Sums of squares	D.F.	Mean square	*F*	*P*
Figures	6·66	1	6·66	1·92	>0·05
Instructions	77·06	1	77·06	22·21	<0·01
Subjects	3829·73	14	273·55	78·83	<0·01
Subjects × instructions	97·94	14	7·00	2·02	>0·05
Subjects × figures	111·34	14	7·95	2·29	>0·05
Instructions × figures	2·41	1	2·41	0·69	>0·05
Residual	48·59	14	3·47	—	—
Total	4173·73	59	—	—	—

interaction but also no difference in reaction time ascribable to figures. Hence, the assumption that receptor control (specified in the hypothesis, '...stimuli which require the same pre-exposure end organ adjustment...') is maintained seems justifiable.

VII. Summary and conclusion

In order further to clarify the issue concerning the contribution of the central nervous system to preparatory sets, three experiments utilizing the reaction time technique were performed to test the following hypothesis: *If an individual is instructed to respond as quickly as possible to stimuli which require the same pre-exposure end-organ adjustment but about which he has different amounts of preliminary information, latency of response will be shorter in those instances in which the preliminary information is greater.*

The results of all three experiments consistently confirmed the hypothesis.

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A GESTALT INTERPRETATION OF SYMPTOMS ASSOCIATED WITH A TEMPORO-PARIETAL LESION

By J. E. A. BARTLET

I. *Introduction* (p. 57). II. *The case* (pp. 57–59). III. *Comment* (pp. 59–60). IV. *Summary* (p. 60). *Reference* (p. 60).

I. Introduction

A report of the following case has been published already by Critchley (1951), but certain features of the case seem to warrant more detailed description and psychological discussion. I refer to perseveration of symbols without perseveration of their surrounding background, in association with metamorphopsia. The latter likewise can be regarded as a disturbance of a 'Gestalt' without disturbance of its 'Figur Hintergrund'. The specific nature of the perseveration warrants individual consideration, but the conclusion drawn may be applied more generally.

II. The case

Mr P. M., a manufacturer's agent, aged 33 years, was admitted to the National Hospital, Queen Square, on 8 September 1950, under the care of Dr Macdonald Critchley, with the complaint of attacks during which he either saw things wrongly or repeatedly or heard things wrongly, and talked unintelligibly. These symptoms had lasted about two and a half months.

No facts that were considered relevant were obtained from the medical history of the patient's family or his own previous illnesses.

Two and a half months previous to admission, while driving a van he noticed the words 'Pullman Springs' in block letters on the back of a van in front. The colours appeared appropriate, although he could not recall what they were. During the next 20–30 sec. on looking at four other vans he saw the same words on each of them, either on the back or side. He was aware of an abnormal occurrence solely by the fact that the sign was reduplicated. On several occasions, while he was driving his van, the same sign, e.g. 'Craig & Thomson', was repeated above several shops, or the same registration number reduplicated on the number plates of several different vehicles. The reduplication might take place simultaneously (e.g. several vehicles seen at the same time with similar number plates) or over a period of time (e.g. several cars seen during half a minute, and possibly going in opposite directions, having the same registration number). During these attacks of visual perseveration there appeared to be no clouding of consciousness, and he successfully negotiated the traffic he encountered.

On one occasion, after having seen the same name over a series of shops, he stopped his van and attempted to write down what he had seen. He thought he wrote down the name accurately, but, when he looked at what he had written several minutes later, he found he had written two decipherable letters and a series of marks consisting of simple geometrical figures. On another occasion the patient noticed that the name on the dial of a clock appeared unusual. He tried to explain this to his wife but only succeeded in producing gibberish. She explained that the word was 'Smith'. He approached the clock and easily read the word 'Smith'. On the evening previous to admission, a man, at whom the patient was looking in a park, suddenly appeared to have acquired greater detail,

significance and importance. On several occasions during the described attacks of metamorphopsia the patient attempted to express himself. He might begin and end with intelligible words, e.g. 'That is' and 'Isn't it?' respectively, but the intermediate sounds were mere jargon with a repetition of something that sounded like 'deva shay'.

Over the one and a half months previous to admission, he had had several attacks when what he was hearing altered in nature. A voice might cease to have meaning, or appear to have a different content from what was actually the case. An example of the latter occurred in the patient's office when he was listening to someone in the room talking on the telephone. Suddenly what was being said appeared to be inappropriate. On asking if the words that the patient thought he had heard had actually been spoken, he obtained the reply 'nothing like that'. On another occasion a musical programme on the wireless suddenly changed into a meaningless debate.

The following attacks were observed while he was a patient in hospital. On several occasions he looked at a plaque on the wall and talked gibberish in which the expression 'deva shay' recurred. The attacks lasted up to half a minute. After each the patient realized that he had seen something unusual but thought he had been describing intelligibly what he had seen. On another occasion a photograph of his wife seemed to stand out in greater contrast and acquire added significance. He had one attack from sleep, when he was observed to sit up in bed and heard to utter a typical group of nonsensical sounds. He immediately returned to his usual sleeping position. Next morning he had no memory of the occurrence, nor of unusual dreams. On 10 September the patient was playing chess with the author, and had overcome a critical situation, when any but one move would have led to a mate. He pointed to his opponent's leading knight, which was approximately on the middle of the board and an important piece at that moment of the game, and said 'That is short a bay ava bay chess', and immediately regained normality. He was able to explain that the chess board in some way became more attractive, and that the pieces stood out more clearly than normally. The knight was seen in excessive relief, particularly the horse's eyebrows. It appeared to have enhanced significance. He did not know what he was trying to say but felt he had explained adequately the unusual appearances. He heard nothing abnormal. I did not speak till after the attack was over. The appearance was that of a minor epileptic attack with altered consciousness. After an intravenous injection of Leptazol, given during the course of electro-encephalographic studies, he had a generalized convulsion at the end of which jargon speech was produced. He had no recollection of visual or auditory phenomena before or during the convulsion.

At no time had there been any feeling of familiarity or 'déjà vu' about the attacks. For the last few weeks before admission, there had been a premonitory symptom consisting of an indescribable sensation inside the scalp at the back of the head. This sensation was always followed by an attack of one of the types described above. The only attack of unconsciousness the patient had had before admission to hospital was a typical faint in 1947. He was right-handed.

His personality prior to his present illness showed no deviations from the normal that were considered significant, nor was there evidence of change of personality since its onset. His prevalent mood was optimistic and cheerful and there was no history of marked swings. His memory for recent and remote events was not impaired and his intelligence was above average. He took a keen but not unduly introspective interest in his symptoms.

He had not suffered from headaches, diplopia, dimness of vision, urinary complaints, weakness of limbs, pains, dysaesthesia, tinnitus or vertigo. General and neurological examination revealed no abnormality. X-rays of skull and chest were normal. An electro-encephalogram showed no significant abnormalities. Lumbar puncture gave a clear colourless fluid under a pressure of 120 mm. of cerebrospinal fluid, containing two lymphocytes per cu.mm. and 0·04 % protein. Pandy's reaction was weakly positive and the Lange colloidal gold test showed no precipitation. There was a normal response to jugular compression. The Wasserman reactions of blood and cerebrospinal fluid were negative.

An air encephalogram was carried out and, although poor filling was obtained, the septum pellucidum appeared to be shifted slightly to the right of the mid-line. The right ventricle did not fill. Cortical air was evenly distributed and normal in amount. A left internal carotid arteriogram showed no significant displacement of the intra-cranial vessels, but there was an appearance of excessive vascularization in the capillary phase in the parietal region in keeping with an infiltrating and space-occupying lesion. An electro-encephalogram carried out after Leptazol showed a 'trigger' area in or around the left temporal lobe from which high voltage 2–3 cyc./sec. waves mixed with 6 cyc./sec. waves emanated. This was followed by the generalized convulsion described above.

It was felt that the history and special investigations indicated that the patient was suffering from an infiltrating lesion of the left parietal or temporo-parietal region. Twenty months after the patient's first admission to hospital a cystic glioblastoma multiforme was demonstrated at operation by Mr Harvey Jackson. It involved a considerable portion of the left cerebral hemisphere including the temporal parietal and frontal lobes. The exact site of the early lesion can only be surmised from the data given above.

III. Comment

There are three describable elements in this patient's attacks—dysphasia, visual perseveration and metamorphopsia.

The dysphasia, using the term in its broadest sense, is episodic, involving the ability to interpret both visual and auditory stimuli. After an attack, although he has produced gibberish or meaningless marks on paper, he considers he has expressed himself adequately either by spoken or written word. Rarely does one get such good insight into the thoughts of such a patient and I feel that there is evidence here that patients with sensory dysphasia not only have an inability to check what they are saying or writing but also confabulate in visual or auditory imagery. This is supported further by the lack of distress typically shown by such patients although they may be quite unintelligible.

This confabulation either in visual or auditory imagery would conform to Wertheimer's Law of Prägnanz, the psychological organization tending to be as 'good' as prevailing conditions allow.

The perseveration of vision on all described occasions was for visual symbols (names and numbers) without involvement of the surrounding background. I suggest that the mechanism is that this patient was suffering from an inability to interpret visual symbols and, owing to the inappropriateness of number plates without legible numbers, and shop signs without legible names, replaced the missing number or name from the most recent suitable visual symbol available to him from his memory, and so perseverated the last seen number or name. I assume that during these attacks (as with the dysphasia, when

he considered his gibberish to be intelligible) he thought that what he was seeing was legible, and I consider this an important factor in causing perseveration in this particular patient.

To sum up, the perseveration attacks were related to a visual sensory dysphasia or asymbolia with visual confabulation causing him to supply the same visual image to a series of similar backgrounds. To apply this conclusion more generally, perseveration may be considered as confabulation related to deficient imagery, the replacing image coming from recent memory.

The metamorphopsia likewise can be considered as a visual disturbance of something without involvement of its background; the disturbance affecting a two-dimensional image (e.g. his wife's photograph) or a three-dimensional image (e.g. the white knight and the man in the park).

I feel that we have evidence that in this patient at least the ability to differentiate an image from its background, or 'Gestalt' from its 'Figur Hintergrund', concerns the parietal or temporo-parietal region of the dominant hemisphere, and probably is closely related anatomically to a region concerned with the conversion of sensory impulses into recognizable auditory and visual verbal symbols, and that the dysphasia and metamorphopsia are similar processes in different types of imagery.

IV. Summary

A patient is described who showed perseveration of visual symbols without disturbance of background, and metamorphopsia, in association with episodic dysphasia, and who is considered to be suffering from a left-sided parietal or temporo-parietal lesion.

The hypothesis that perseveration is confabulation replacing deficient imagery is put forward.

It is suggested that in this case the function of differentiating 'Gestalten' from 'Figuren Hintergründe' is dependent on the left temporo-parietal region and is related anatomically to an area concerned with the interpretation of visual and auditory verbal stimuli.

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PSYCHOLOGICAL PROBLEMS ARISING FROM A REPORT OF TELEKINESIS

By E. J. DINGWALL

I. *Introduction* (pp. 61–62). II. *An unpublished record* (pp. 62–64). III. *The choice among explanations* (pp. 64–66). *References* (p. 66).

I. Introduction

For students of abnormal psychology the more or less commonplace phenomena of hallucinations are, perhaps, the least understood of the obscure material with which they have to deal. Often linked as they are with psychotic symptoms it was some time before it was fully realized that they played a part in normal life and then that they had to be sharply distinguished from the more easily understood illusions of the senses. Indeed, apart altogether from the well-known hypnagogic and hypnopompic hallucinations, the occurrence of even the more complex hallucinatory phenomena has been noted among persons who certainly could not be called insane, although in these cases such phenomena were rarely experienced many times during the subject's lifetime.

With the coming of mesmerism and the hypnotic 'sleep' it was possible to embark on a new technique for the experimental production of hallucinations, but these experiments were limited in scope and did not touch a number of hallucinatory phenomena. Moreover, it was very difficult to determine how far a hypnotized subject experienced such visual hallucinations as those, occurring, for example, with Nicolai or Staudenmaier, as it was possible that the subject was merely behaving as if he were experiencing the situation suggested by the operator. In spite of these limitations, however, moderate advances in our knowledge were made, and the attempt to show that certain classes of these phenomena appeared to be connected with events normally unknown to the percipient pointed the way to a closer analysis which, it was hoped, might throw light upon their genesis and meaning. Moreover, it was soon seen that hallucinations might provide an explanation for some of the more startling phenomena reported as occurring with certain mediums, such as the levitation of objects and apparent movements without contact. Similarly, it was recognized that certain phenomena widely reported among Roman Catholic communities, such as moving pictures, bleeding statues and apparitions of holy persons might all be classed as hallucinatory, although presenting features which are still little understood and which sometimes appear to violate the rules commonly supposed to be linked with the emergence of these perceptual disturbances.

Attempts to show that many of the physical phenomena of mediumship were due to visual or auditory hallucinations were not particularly successful. It was easier to explain them by fraud, malobservation or lying, as all these factors were known to be of common occurrence. Those versed in the psychology of testimony were aware of the numerous sources of error (such as faulty recall of an observed act) which had to be avoided, and advances in photography indicated that many of the phenomena, whatever their nature and explanation, were objective and not due to any kind of hallucination on the part of the observers. In the case of the more startling and better attested phenomena, especially

where the theory of fraud seemed difficult to accept, it was often suggested that collusion on the part of the observers or even the investigators seemed the best explanation. Unfortunately, these cases were reported at a time when photographic technique, especially in the dark, was not much developed, and thus no satisfactory photographic records of the more complex phenomena are extant. All we possess is a consensus of opinion among a large number of men and women, distinguished in various walks of life, that they had certain experiences which they described often in identical terms and which they were completely unable to explain to their own satisfaction.

Among persons in whose presence these well-nigh incredible events were reported was the medium D. D. Home (1833–86), one of the most enigmatic personalities of the nineteenth century. For some twenty years he was lionized by literary and artistic circles in England, giving dozens of séances at which were present such prominent people as the Brownings, the Cowper-Temples, John Bright, Ruskin and many others (1). In France he gave many sittings to Napoleon III, and the letters of the Empress Eugénie (2) bear testimony to the wonder he caused in court circles. The same story was repeated in Germany and Russia. Everywhere he went similar phenomena were reported as occurring in his presence. Tables rose in the air in well-lighted rooms and knocks resounded: sometimes armless hands were seen, touched and grasped by the observers, who reported that when the grasp was maintained the hands dissolved leaving no trace.

One difficulty hitherto found in appraising these reports is the fact that very few contemporary and fully detailed accounts, compiled by persons above reproach and with keen powers of observation, are extant. Most of the material is anecdotal, composed by persons with little knowledge of or even interest in scientific procedure, which in those days was not sufficiently developed to deal with such problems. If we omit the experiments that Mr (later Sir) William Crookes had with the medium, we can say that the testimony regarding Home's powers are merely accounts by untrained and often credulous observers of what they saw and heard at the séances they attended. It is, however, true to say that they are usually in complete agreement as to what they did see. The phenomena observed, whatever the explanation, were described in the same way in England and the Continent. What was lacking were the details which might enable us to judge how far credence could be placed in the accounts and how far it was possible, on the existing evidence, to explain the observations on the basis of hallucination, fraud or confederacy.

II. An unpublished record

A short time ago information reached me that a manuscript dealing with D. D. Home was in existence in the library of the Earl of Crawford and Balcarres and that it contained material of very considerable interest. Through the courtesy of Lord Crawford I am now able to draw the attention of psychologists to this manuscript. It contains one of the most detailed accounts of the phenomena of D. D. Home that we possess and, owing to the character of the narrator and his companions, the record does not seem to me to suffer from the same doubts that are to be felt about so many other contemporary accounts.

Owing to pressure of space I propose to deal with only one of the manuscripts in the volume. It is a lengthy letter from the 25th Earl of Crawford (1812–80) to his sister-in-law,

Mary Anne Lindsay, the daughter of Lieut-General James Lindsay of Balcarres (*d.* 1885). In this letter are described some sittings with D. D. Home in Florence in 1856. These sittings took place in a villa belonging to Major Charles Gregorie, formerly of the 13th Regiment of Light Dragoons, who lived there with some relations, a Mrs Crossman (widow of a clergyman) and her two adult daughters, Miss Crossman and Mrs Baker. Meeting Robert Lindsay, brother-in-law of Lord Lindsay (as the 25th Earl of Crawford was at that time) at the club, Major Gregorie invited him and later Lord Lindsay and his wife Margaret Lindsay to visit him at his villa and be present at a few sittings with Home, whose phenomena had puzzled him completely. Lord Lindsay and his party accepted this invitation very willingly, and he begins his letter by saying that it will be a 'letter of marvels' for '*our* ghosts have all appeared in broad lamplight and around the tea-table and their manifestations and communications have been unaccompanied by any of the usual apparatus of humbug and delusion'.

Of the three sittings described by Lord Lindsay, he himself was present only at the last, the records of the first and second being taken down almost immediately afterwards from Robert's own lips. We have, therefore, records of three séances of which the details were written out immediately after the sittings and of which one is a first-hand account by Lord Lindsay himself.

The content of the letter reveals the writer as a remarkably careful and acute observer. It is full of vivid and meticulous detail regarding Major Gregorie's house, the history of his relatives, the appearance and family story of the medium and the nature of the phenomena observed. Of these phenomena I intend to choose but two in order to illustrate the kind of experience to be explained and the difficulty of accepting the theory of hallucination, a theory discussed in the letter and rejected as quite inadmissible.

The sittings were held in the drawing room, the party being seated round a heavy table 'of the size at which seven people can sit comfortably in a circle. It had a cloth upon it, hanging over on all sides about three inches'. On this table stood one of the Carcel oil lamps in common use at the period, and Robert Lindsay records that it was at the first sitting that he observed the levitation of the table which was constantly observed at Home's séances. Lord Lindsay writes: 'After the party had sat down to the table and placed their hands on it, tappings were heard immediately upon the table and on all the tables in the room, very violent—the table then began to tremble—swayed backwards and forwards—then suddenly and violently rose up from the ground to the height of five feet, higher than the heads of the persons sitting at it, and as violently descended—but was redeposited on the floor as softly as a feather.... There was nothing under the table for Robin [i.e. Robert Lindsay] stooped down and crawled under it.'

At the end of the sitting, the company got up, the lamp was removed and tea brought in. While they were all talking round the fire, 'suddenly a marble table at the further end of the room—that is a table with a loose marble slab on the top of it—violently rose up to the height of three feet, and redescended in the same manner, and also tilted over—while the slab, and a pencil and paper which lay upon it, remained stationary. It bent almost down to the ground, and when Robin asked it questions, it answered by three plunges for 'Yes' and one for 'No'. Robin tried with his utmost strength to force it back into its natural position, and had the greatest difficulty in doing so.'

At the third sitting, at which the writer himself was present, he declined to sit with the circle at the table as he thought that if he were outside he might not be influenced by any

suggestions, since he says he was aware of the explanation which maintained that the phenomena were subjective.

After the rest of the circle had taken their places, with Home sitting between Mrs Baker and Miss Crossman, the usual manifestations occurred. Taps began almost at once on the under side of the table, and then 'the table began to vibrate, and then the chairs; and then the floor and then the whole room trembled and shook, while the china rattled on the table at the further end of the room'.

On looking under the table Lord Lindsay saw nothing except the feet of the persons present [and presumably the central leg which supported it]. But immediately afterwards the table 'rose suddenly straight up to the height of four feet—remained suspended in the air for about half a minute, swaying about in different directions—I again looked under the table, while it was moving about, but there was nothing visible—and then came down again quite gently....'

It will be observed that the experience that Lord Lindsay had was very similar to that previously reported to him by his brother-in-law, Robert Lindsay. The large table gave them the impression of rising three or four feet into the air, and in the case of Lord Lindsay it is said to have remained suspended for about half a minute. Similarly, the marble-topped table was reported by Robert Lindsay to have risen into the air when nobody was very near it, and it was with great difficulty that he forced it to the ground.

III. The choice among explanations

Now if these phenomena occurred at all (and they are similar to those observed with Home in other parts of Europe and in England) then they must have been produced either by normal or by some other means. If by normal means, then it is quite clear that Home must have induced one or more of the party to act as his confederates in order to deceive the rest of the company. This appears to me very unlikely when we consider the nature of the company and the character of the event observed. As every conjurer knows satisfactory levitation of tables without detection under these conditions is very difficult, and in the case of this heavy table it would have been clearly impossible for Home to have done it unaided and the confederacy of either Major Gregorie or Robert Lindsay or both would have to be assumed. Even if we go so far as to assume this to be the case, it does not begin to explain the movements of the marble-topped table reported by Robert Lindsay. Moreover, if we assume confederacy in order to explain these table levitations in Home's presence, we must, I think, put forward the same explanation for most of the other occasions on which the same phenomena were reported, thus dragging in a considerable number of confederates not one of whom ever confessed to having thus assisted the medium. Since during the whole of Home's active life, there was the most violent controversy both over his phenomena and his character, and intense jealousy on the part of the many fraudulent mediums whom he castigated so soundly in one of his books, it is, to say the least, rather curious that no reliable witness came forward to confess to active assistance during any of his séances at any time.

It would seem, therefore, that the theory of fraud coupled with collusion can hardly be sustained. We are, it seems, driven a step nearer to the theory of hallucination which Lord Lindsay found himself unable to accept. This would imply that Home was able to influence persons he had never before met and, without their knowledge, and without

any of the usual preliminaries to hypnosis, to induce in them visual and tactile hallucinations which they believed at the time to be objective experiences. Now, if this be possible I have never heard of any experimental evidence, even with the most powerful hypnotist, to support it. It may be urged that such phenomena as moving pictures and bleeding statues indicate that visual hallucination under certain conditions can be generated in ordinary persons without resort to any hypnotic procedure and that Home's phenomena were merely another example of the same process. It ought, however, to be pointed out that these obscure visual hallucinations are very different from those which, on this theory, occurred with Home. For in the latter case there occurred not only visual but auditory and tactile hallucinations, unless it be supposed that the phenomena were due to a combination of a number of factors such as fraud, etc., coupled with visual hallucinations. It will doubtless be urged that in the case of the Indian Rope Trick, it has been shown that such hallucinations can be induced in apparently normal spectators and that this miracle can be legitimately compared with those of Home. I am, however, far from satisfied that any of the stories of photographing the Rope Trick with ordinary and ciné-cameras (cf. de Croisset (3)) are reliable; I believe that simpler explanations would suffice. Moreover, it would have to be assumed that, in the case of Home, scores of persons taken from the educated classes of Europe fell under the same influence and that on no single occasion did one sitter maintain that, while others saw these most remarkable occurrences, he (or she) saw nothing whatever. Although I know of one case resembling this (cf. Dingwall (1), pp. 127–8) it had features which sharply distinguish it from those present with D. D. Home, but it is true that in a number of instances when the armless hands appeared and were seen to move material objects, some of those present stated that they saw a hand moving the object, others saw only a luminous appearance and others merely saw the object moving through the air. In these cases, however, the details are not sufficient to enable us to ascertain with any degree of certainty how far this conflict of opinion may have been due to varying acuity of vision in the observers, or to different angles of vision from which the phenomena were observed.

The importance of the Earl of Crawford's manuscript lies in the fact that through it we seem forced to choose between a very limited number of alternatives. If hallucination be accepted then it is certainly not a kind of hallucination of which we have any detailed knowledge or, indeed, any experience whatever. But in the case of the marble-topped table the only three explanations appear to be that Robert Lindsay invented the whole story with the agreement of the others, or that both he and the rest of the company were suffering from hallucinations of an exceedingly peculiar and unknown kind, or that the event occurred as described and remains inexplicable in the present state of our knowledge.

Whatever may be the explanation preferred, it is obvious that when the opportunity occurs further investigation with adequate photographic technique is urgently required. For if the theory of hallucination be the true one, then an inquiry into the scope and character of such suggestion must throw much light on the psychology of testimony. Since we know of no medium to-day comparable to D. D. Home (whose phenomena were as different from those of the miserable frauds of his day as they are from those of our own times) it would seem likely that an examination of one of the many cases of moving pictures and the like might yield material of much value. It is much to be regretted that so many psychologists turn away from these problems as unworthy of attention, although

it is true that a full appreciation of them involves the acquisition of knowledge and a practical experience that few psychologists possess or are willing or even able to gain.

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(*Manuscript received* 17 *May* 1952)

CORRESPONDENCE

The Editor
British Journal of Psychology

Dear Sir,

Readers of the review of the *Scientific Session on the Physiological Teachings of Academician I. P. Pavlov* by K. R. L. Hall in the August number of the *Brit. J. Psychol.* will receive rather a distorted picture of the contents of this publication, as this seems to be a review of London's account of 'Contemporary Psychology in the Soviet Union' (*Science*, 1951, CXIV, 227–33) rather than of the volume proposed for review. The reviewer has chosen to adopt what seems now to be the traditional attitude to Soviet discussions in Western countries, namely, to apply standards of journalism rather than scholarship to these phenomena. This entails so obscuring the real object with such a cloud of self-righteousness, moral indignation, oblique or direct references to the reinstatement of outmoded trends by piecemeal equating of quotations out of context, Cassandra-prophecies of the future effects of what the reviewer or critic takes to be the latest fiat of the wicked men in the Kremlin—that any approach to an understanding of these discussions must begin by an emptying of the mind of everything one has read about them by Western commentators.

May I take issue with only the most misleading statement of your reviewer: his assertion that there is a 'vehement rejection of the "subjective" method'. This, from the context, is a reference to Bykov's criticism of Orbeli (pp. 39–41) for belittling Pavlov by assigning priority to Hering. There is nothing here which could be interpreted as a rejection, vehement or otherwise, of the 'subjective' method. Bykov is denying, as Pavlov also denied, that the objective methods of physiology and the method of self-observation should be considered as having equivalent status. This seems to me to be eminently reasonable, and to be in accordance with normal psychological procedure. The theoretical justification of the exclusion of subjective methods from *physiological* investigations, developed in some detail by Ivanov-Smolenski (pp. 119–133 and 143–146), if understood in the full context of the Pavlov discussion, which is not completely reported in this volume, gives no support to the view that psychology has lost the independent position it gained for itself in the Soviet Union in the period 1917–29. If anything, its position has been strengthened as the result of this discussion, one of the main features of which is the clarification of methodology appropriate to psychiatry, psychology and physiology. It is perhaps superfluous to point out, in view of the material available in English, especially the most useful survey by London (*Psychol. Bull.* 1949, XLVI, 241–77), that psychology has a very different content and performs a very different function in the Soviet Union to those varieties of psychology with which most of us are familiar. However much we may regret this fact, we must accept it as our basic starting-point in attempting to understand the developments in Soviet psychology, and not allow our interpretation to be wholly coloured by wishful thinking or other irrelevant considerations.

Yours faithfully,
John McLeish

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30 *August* 1952

THE EDITOR
British Journal of Psychology

Dear Sir,

Mr McLeish, having abreacted rather fully and satisfyingly in the first paragraph of his letter, proceeds to make one or two statements which I should be glad to discuss further.

First, it is a curious thing that your correspondent should seem to have committed himself completely and uncritically to the side of the orthodox Pavlovian speakers in this session, with whose views, therefore, he would appear to identify himself. My review was, indeed, to some extent concerned with criticizing just that dogmatic Pavlovianism shown in these speeches in so far as it seems to be restrictive of original physiological and psychological work that has been carried out by other Soviet scientists. In other words, your correspondent is, shall we say, 'vehemently' supporting one set of Soviet views which happen to be in Party favour at the moment. One might get the impression, however, from the tone of his letter that he is rising impartially to the defence of *all* Soviet physiology and psychology, as though all aspects of the Soviet contributions to these sciences were being condemned unfairly and with Western prejudice merely because they happen to emanate from the U.S.S.R.

This, I think, is a point which your correspondent, in his zeal to defend the current Soviet pronouncements from attacks of any kind, has quietly overlooked. If I, for example, choose to defend the point of view of Orbeli, who is, or was, head of the Pavlov Institute in Leningrad, and of Rubinshtein, whose general psychology text-book won a Stalin prize in 1942 before it came under heavy criticism for inadequate Pavlovianism, it seems rather paradoxical that such a defence should be identified with a 'traditional attitude to Soviet discussions in Western countries'. If one happens to regard certain aspects of Soviet psychology as quite outstanding, whereas other recent developments seem sterile, it is strange that one's selective interest should be condemned apparently on political grounds. If, instead, I had strongly criticized some outmoded and retrograde behaviouristic trend in the U.S.A., I can hardly believe your correspondent's reaction would have been quite so intense.

Secondly, your correspondent takes exception to the statement that there is a 'vehement rejection of the "subjective" method' in the Pavlov session speeches. There seems to me to be a slight inconsistency in his argument here, for at one moment he is denying that this volume does contain 'a rejection, vehement or otherwise' of the subjective method while the next moment he says that the rejection of the subjective method 'seems to me to be eminently reasonable and to be in accordance with normal psychological procedure'. Well and good, he has doubly covered himself. Now, I would certainly agree that the subjective method is unworthy of equivalent status to objective observation, say, in physiological research on digestion. But the spread of the physiological teachings of Pavlov is repeatedly and specifically stated to provide a scientific basis, not only for physiology but for psychiatry and psychology as well. It is to be the basis, not only for the study of the simpler mental reactions but of all those complex mental functions involved in human learning and thinking. Assuming, therefore, that Bykov's criticisms of Orbeli and Ivanov-Smolensky's remarks do constitute a rejection of the subjective method, it is perhaps not unreasonable for the reviewer to draw attention to

what seems to him a methodological inadequacy reminiscent of an early and naïve behaviourism. Vygotsky long ago pointed out, in criticizing Pavlov's work on conditioning, the inadequacy of his purely objective approach for the complete understanding of complex mental functions. Pavlov himself no doubt might have agreed, but now such criticism is deemed reactionary and therefore bourgeois and therefore unacceptable.

Mr McLeish concludes his letter by pointing out that the content and the function of psychology in the U.S.S.R. are very different from that with which most of us are familiar. Most of us, or at any rate those who have read London's surveys of Soviet Psychology, would agree with this statement. Many people might feel, from reading London's historical survey covering the period 1917 to 1931, that the development of a distinctive school of 'dialectical psychology' was significant and important. But, in his survey of 'Contemporary Psychology in the Soviet Union', London is able to give a much wider background, from other Russian sources, to the trends apparent in the Pavlov sessions under review. It is the stress on conformity to a particular doctrine, in this case orthodox Pavlov doctrine, which is so completely divergent from any conceivable policy directive in this country. A fiat from the Council of the British Psychological Society advocating strict adherence to orthodox Spearmanism, for example, would be rather unpopular in some departments in this country, while a rejection of Gestalt principles, not in consequence of experimental investigations but because of theoretical incompatibility with some philosophical tenet of the party at present holding office, would seem very strange to us.

It is for reasons of this kind that it may sometimes be difficult not to be blinded to the possible excellence of much of the individual work carried out in the U.S.S.R. No doubt the 'heretics' of the moment manage to continue their own investigations, even after recantation. Nevertheless, one is entitled to doubt whether the mental discomfort of renouncing what one believes to be important in one's work in favour of a valuable but now inadequate set of principles is conducive to good and original research in psychology or any other science.

Yours faithfully,

K. R. L. Hall

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Barrow Gurney
Near Bristol
17 *September* 1952

The Editor
British Journal of Psychology

Dear Sir,

I note with regret in the *Brit. J. Psychol.* the review of the recent book *Scientific Session on the Physiological Teachings of Academician I. P. Pavlov.* The review provides the reader with little information about the substance of the book, and some of the points made tend to be misleading.

It seems to me that these discussions of the Soviet physiologists are of interest to us, and for several reasons.

One of the chief merits of the book is that it shows how limited has been our own appreciation of the significance of Pavlov's work and theories. The Soviet physiologists have at their disposal of course much of Pavlov's work which is not available to us in English. This gives them the greater opportunity to understand his theory and its implications.

They make their assumptions clear; they have chosen to base their work on that of Pavlov because they regard it as consistently scientific and materialist. They consider that his theory of the higher nervous activity advances our understanding of the major biological problems: the theory of evolution, the interaction between the organism and its environment, hereditary and acquired experience, and so on. They consider also that it provides for a scientific approach to the old philosophical problems: the nature of the subjective, the mind-problem, free will. Of particular interest is the discussion on the subjective, on thought and speech from the Pavlovian point of view.

A recent review in another British journal mentions 'the stagnant atmosphere of contemporary psychological discussion'. It is disappointing if the points made in this book are not to be discussed further.

Yours faithfully,

MARGARET A. VINCE

15 King's Parade
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26 *October* 1952

THE EDITOR
British Journal of Psychology

Dear Sir,

The much wider, almost universal, significance given to Pavlov's work and theories in these discussions is certainly an outstanding characteristic of this book. Whether this is a chief merit, as Miss Vince contends, or a chief defect, at any rate in its implications for psychology, is a controversial issue too wide for further elaboration here.

I would, however, like to discuss in some detail her view that the references in these Proceedings to the Pavlovian theory of thought and speech are of particular interest. Perhaps it will be as well to outline very briefly a few of the main points that the speakers make. These are:

(1) Vavilov (pp. 13 and 14) specially applauds Pavlov's theory of the second signal system, because he says it opens up quite new paths for a study of language from the standpoint of natural science.

(2) Bykov (p. 73) speaks of this theory as the pinnacle of Pavlov's creative daring, and quotes the following passage from Pavlov's *Complete Works*, vol. III: 'If our sensations and ideas relative to the surrounding world are for us the first signals of reality, concrete signals, then speech, especially and primarily the kinesthetic stimuli which proceed from the speech organs to the cortex, constitutes a second set of signals, the signals of signals. They represent an abstraction from reality and permit the forming of generalizations, which constitute our extra, specifically human, higher mentality....'

(3) Although this is considered to be so high an achievement, Bykov and Vavilov regretfully admit that exceedingly little is being done in the U.S.S.R. to develop its

implications. In fact, valuable work has been done on language and thought by Vygotsky, Luria and others, but presumably it has not, until recently, been obligatory to set this within the general framework of Pavlov's theory. In other words, such work as has been done on thought and speech has owed little, if anything, to the Pavlov theoretical background.

One is obviously handicapped, as Miss Vince points out, in the appraisal of such a theory, as of the other controversial issues, if one does not have access to the actual publications upon which the speakers rely. Nevertheless, from what is set down in the Proceedings, I have the impression, possibly erroneous, that this second signal system theory is nothing more advanced than the kind of formulation which Watson makes in his early publications on Behaviourism.

In an attempt to universalize Pavlov theory so that it fits the most complex mental functions, and to avoid indebtedness to non-Soviet writers, it seems to me the speakers tend to restate with emphasis, as though it were new, a theory as old as behaviourism itself. It is for this reason that, rightly or wrongly, more detailed reference to it was not made in the original review.

Yours faithfully,
K. R. L. Hall

Barrow Hospital
Bristol
2 *November* 1952

CRITICAL NOTICE

By JAMES DREVER

The Scientific Study of Personality. By H. J. Eysenck. London: Routledge and Kegan Paul. 1952. Pp. xiii+320. 30*s*.

Dr Eysenck has a laborious and peremptory way with him which will repel many psychiatrists and some clinical psychologists. Nevertheless, they should all read this book. If they disagree with Dr Eysenck's methods and conclusions then they should give their reasons as clearly as he has done, and suggest alternatives. Otherwise their case may go by default. It is no use mobilizing resistances or brushing up alibis. The whole argument is central to the clinical field. It may conveniently be divided into three stages. First of all Dr Eysenck states certain implications of our traditional scientific logic relative to the psychology of personality; then he puts forward statistical procedures worked out in the light of these implications; and finally he shows how in fact these procedures have given rise to new knowledge of a scientific kind in the fields in which they have been used. The researches referred to include his own earlier work, and that of his collaborators as well as a good deal of recent American work along similar lines.

The first step of the argument embodies two claims; that science should use the hypothetico-deductive method, and that measurement is necessary for the verification of its deductions with a known degree of probability. Stated thus Dr Eysenck's position seems a strong one. The scientist must be able to specify the data relevant to his generalizations, and, in current jargon, he must have some system of feedback to let him know whether these generalizations fit. Otherwise he is not so much groping in the dark as gesticulating in a vacuum. At the same time we must keep in mind the proviso that scientists, particularly successful ones, often seem to break the rules that the logicians lay down for them.

It is when he comes to particularize that Dr Eysenck begins to leave an occasional loophole. Consider his attacks upon holistic theories and their allied techniques. He is right in pointing out that all psychologists 'atomize' or break down the total personality whether or not they may claim to treat it as a whole. He fails to make it clear, however, that this breaking down can follow very different lines. He instances the Freudians and their 'Oedipus complexes and super egos, regressions and transferences, cathexes and libidos, fixations, symbolisms, compensations, catharsis, Narcissisms, and many other strange entities jostling each other, all of them attempts to analyse what is elsewhere declared to be "unanalysable"'. This list is in itself rather an unfair hotch-potch, but that is not the point. What Dr Eysenck appears to imply is that its contents can be compared with descriptive traits such as neuroticism or extraversion as though they were the same sort of thing but not nearly so 'scientific'. This is just misleading. To take a parallel case: One may describe a car in terms of its cylinders, its pistons, its ignition and its transmission, deriving these, if you like, as constructs from its 'behaviour'. One may also describe it in terms of its horse-power, its weight, its mechanical efficiency, and so on. In terms of these two descriptions one may 'explain' the motion of the car in two ways: either in terms of what happens inside it or in terms, say, of its horse-power/weight ratio. There are other possibilities both in psychology and engineering. They are complementary and may be equally valid. In this case the former approach may seem the more useful of the two, but the question of usefulness is not really relevant here. If someone were to say 'I have a car. Its horse-power is x and its weight y. It won't go. What shall I do?' Dr Eysenck could fairly reply 'Sir, I am a physicist not a garage mechanic'.

There is the further point of course that in the case of Freudian theory, whatever one may feel about its possible usefulness, there has, as Dr Eysenck points out, been rather a dearth of the sort of empirical studies that would enable us to choose between sparking plugs and gremlins. Nor is Freudian theory alone in this. All the applied psychological enterprises stand in need of more formal validation than has in general been accepted so far. A good deal of work in this field is already going on, though perhaps with a less explicit theoretical framework than Dr Eysenck would like.

The second stage in Dr Eysenck's argument has to do with the problem of measurement in the psychology of personality. 'If science depends on measurement,' he says, 'then we must know what to measure', and this requires the establishment of operationally defined dimensions. These dimensions can be—indeed must be—in a sense arbitrary or conventional, and personality may be measured in terms of different sets of dimensions so long as some technique of translation from one to the other is available. Dr Eysenc

himself feels, however, that the experimental facts will gradually prune the alternatives that inventive psychologists might think up or derive from their correlational matrices. This means that, although they are in one sense conventional, in another the dimensions of personality derived from this kind of study are real and objective.

This process of pruning is clearly crucial, and to consider it one must turn to the logic of the statistical techniques which Dr Eysenck proposes. Starting with an exposition of classical factor analysis he traces the reasons which led Thurstone to modify the orthogonal pattern of factor axes and to allow inter-correlation between factors. He then goes on to point out why 'simple structure' is not likely to reveal itself in such a new and complex field as that of personality testing. Finally he puts forward criterion analysis as a method which may be used to determine the position of oblique axes in a manner conforming to the general hypothetico-deductive method of science. The procedure is most ingenious. If we regard a factor as a sort of average then we can give it meaning by determining through our choice of tests what is in fact being averaged. This calls for some prior knowledge of what constitutes, say, neurotic behaviour or extraverted behaviour. We can then use our tests on groups classified normal-neurotic or introverted-extraverted as the case may be. By correlating each test with this dichotomous criterion we can add a 'criterion column' to our matrix, and show the correlation with it of our factor saturations. By moving our axis slightly we can then bring this last correlation to a maximum thus making our factor more or less invariant. Having done this we can turn back upon our tests and set about improving them as measures of the factor with which we are concerned, and so in turn we can improve our criterion until the whole procedure reaches a high degree of efficiency.

The obvious criticism, and Dr Eysenck deals with it at some length, is that we are arguing in a circle. At one end we use some general psychiatric views as to what constitutes neurotic behaviour, and at the other we use a psychiatric diagnosis to obtain our two criterion classes. Dr Eysenck's claim is that such apparent circularity derives from the necessarily conventional nature of measuring. He points out that a procedure very like his own is used by physicists to establish measures of length or temperature. Here rough and ready techniques lead to a first generalization in terms of which the techniques can be improved, thus making possible an increase in the scope and precision of the generalizations. The analogy appears to hold with the exception that in the physical fields there seems possible at the outset a basic pointing operation to something given as dimensional. A similar operation may be possible for intelligence but neuroticism seems more doubtful. The operation by which Dr Eysenck places normals and neurotics on a continuum is not one of pointing. The clinician might have an answer here if he could understand what he was being asked. As it is, while one cannot say that Dr Eysenck has smuggled in the rabbit which he later produces, there does remain a suspicion that at some stage in the proceedings he has left his hat lying about in a rabbit warren.

These theoretical considerations, however, comprise only a small part of the book, and its author would no doubt claim that it should be judged primarily by its factual contribution. Here the material presented and referred to is impressive. Dr Eysenck and his associates during the post-war years have carried out a co-ordinated series of researches which achieves much and promises more. The scope of the work may be limited but the limitation is deliberate. At least it is explicit both in its methods and its findings so that anyone may repeat the investigations and confirm or modify the results so far obtained.

In the book under review the factual matter consists first of all in an analysis of existing research on neuroticism from which is derived an operational definition of the neurotic dimension. Then follows an account of the various techniques of measurement and the results to which they have given rise. Some readers may feel that the assortment of tests, inventories and apparently trivial antics which is used cannot possibly measure a single variable. Objections along this line are not valid, or at least could only be validated by further experiment.

Having defined and used his measuring instrument, Dr Eysenck returns again to the question of the 'reality' of the neurotic dimension. One would say that he shares, in an open-minded and self-critical way, the suspicions expressed about the provenance of his rabbit. What he tries to do is to break out of the circle, if circle it be, and show that what he is measuring varies in terms of some quite independent factor, in this case a genetic one. Is there a difference in terms of neuroticism between fraternal and identical twins? The hypothesis that such a difference does exist is provisionally verified and whatever is being measured—neuroticism, emotional instability and lack of integration are equivalent verbal forms—thus acquires a biological as well as a clinical anchor. Further investigations here are clearly of great practical as well as theoretical importance.

Dr Eysenck next turns to the psychotic dimension, using Kretschmer's later work as his starting-point. The dimensional approach does seem to tidy up the diagnostic situation here. If a psychiatrist is only

entitled to use one label on a patient whose condition varies in a number of dimensions then clearly he has an impossible task, which can only be attempted in a qualitative unsystematic verbal way.

Other applications of this method are given in a chapter which deals with its employment in the educational and industrial fields as well as its more familiar clinical use.

Finally, a return is made to general topics in a comparison between the organismic and analytic ways of studying personality. Dr Eysenck attacks, in particular, the claim that so many complexly related factors operate in the determination of behaviour that only a skilled clinician can make the necessary assessment and integration. If the factors can be identified, he says, their most effective integration can be mathematically determined. The clinician cannot hope to do better and he may not do as well. There seems a way out in that the mathematical integration holds for a group in general, whereas intuitive variations might produce more satisfactory diagnosis or selection in the case of individuals. Unfortunately, recent work shows that clinicians and selectors do not improve upon tests, and may even impair their value, by using interview procedures. It is a pity that we should simultaneously have to cope with evidence suggesting, on the one hand, that tests are our most reliable diagnostic tools and, on the other, that the most reliable kinds of tests are not very reliable. The apparent sensitivity of tests to all sorts of environmental factors makes the repetition of this work by other investigators in other centres an urgent task. Any attentive reader will find himself asking questions; for instance, are hysterics really less suggestible than the average? Even to look at the pictures raises the query, how much variance is due to the charm and efficiency of the testers and how much to the tests. Postgraduate students would be far better checking points like this than thinking up some bright irrelevance of a Ph.D. subject. Devising new projective techniques is almost an occupational disease among them just now.

It was suggested at the outset that all psychiatrists and clinical psychologists should read this book. Most of them will be well advised to read it at least twice.

PUBLICATIONS RECENTLY RECEIVED

Social Psychology. By Robert E. L. Faris. New York: Ronald Press Co. 1952. Pp. vii+420. $5.00.

This book is intended for use with University courses in social psychology. It includes treatment of such subjects as motivation, rôle behaviour, personality disorganization, and the social factors that influence learning, perception, memory, attitude, belief and ability. There are a number of somewhat fundamental weaknesses. The social psychologist who has successfully freed his subject from the 'group mind' and similar concepts, is unlikely to welcome the thesis developed that 'the characteristics of a social organization are not entirely unlike those of a biological organism' (p. 7). Again, he may remain somewhat unsatisfied by an analysis so rigidly isolated from neurology, genetics and the other *biological* sciences. To these psychology—even social psychology—must have reference, and with these it must achieve eventual integration. For example, Freud and Pavlov receive little sympathy. Objection is taken less to psycho-analysis and reflexology as such, than to their common appreciation of the part which 'biological' factors play in motivation. Again, even in relation to 'ability', the influence of genetic components, is severely discounted. There is a certain reluctance to admit not merely 'genetic influences' but even 'physiological processes', and the two are sometimes a little confused! This results in rather too selective treatment of some of the authorities cited. For instance, though D. O. Hebb and Adelbert Ames receive frequent mention, little indication is given of the strong emphasis which both place upon the close inter-relations of the physiological and the psychological. The book is clearly written and might be recommended for reference purposes. Its most serious limitation is a tendency to over-statement and a lack of perspective towards cognate sciences other than sociology. This weakness limits its usefulness as a formal text-book in social psychology. P.MCK.

Studies in the Social Psychology of Adolescence. By J. E. Richardson, J. F. Forrester, J. K. Shukla and P. J. Higginbotham. Edited by C. M. Fleming. London: Routledge and Kegan Paul Ltd. 1951. Pp. x+266. 21*s.* net.

This book is divided into three parts. The first, comprising more than half of it, consists of a paper by J. E. Richardson about the social relationships and changing attitudes within two groups of girls in a secondary school during their English lessons. Group work, when the girls were allowed to choose their workmates, not only resulted in greater achievement than did more formal class instruction, but gave the teacher clearer insight into their developing social attitudes and at the same time had favourable therapeutic effects on solitary, unpopular and over-aggressive children. Comparatively few readers will be interested in a lot of the detail, but the planning of the investigation and the use of Moreno's sociometric techniques in a classroom situation are worth studying by teachers who have the desire and the opportunity to experiment.

Part Two is called 'Adolescent Opinion'. A short paper by Forrester describes the use of a test to discover the attitudes of boys and girls towards certain aspects of their own development. The discussion of the form of the test is useful and there are some interesting results briefly reported. Shukla's paper is a study of friendship among secondary modern school pupils by means of questionnaires and rating scales.

Part Three, on Group Work and Classroom Attitudes, consists of a paper by P. J. Higginbotham on leaderless discussions by groups of grammar school and approved school pupils. The amount of rather dull reported speech in this paper and the complex charts and sociograms give it a somewhat forbidding appearance but the dynamic factors involved, the resolution of tensions and the opportunities available for useful diagnosis and therapy are clearly illustrated.

All four papers make extensive use of modern methods of social investigation—attitude tests, questionnaires, sociometry, and of the vocabulary and concepts of Moreno, Lewin and their followers. Reports on explorations in this field with new methods are valuable, even if no very important conclusions are reached. It seems, to the reviewer, a pity that these studies of pupils at the secondary school stage should be presented as researches in adolescence without any reference to the pubertal status of the subjects. A.H.D.T.

The Earliest Stages of Delinquency. By H. Edelston. Edinburgh: E. and S. Livingstone Ltd. 1952. Pp. vii+200. 10*s.* 6*d.*

The difficulty about a book like this is that it ought to consist of at least 800 pages; however, Dr Edelston has written a useful book for members of other professions than psychology or psychiatry who want to know what is the middle-of-the-road view of the causation and treatment of delinquency, and those responsible for study groups, extra-mural courses, W.E.A. lectures, etc. will find it a serviceable and sensible book on the subject.

The Child Guidance Clinic is made to carry rather more than it can at present manage; all the diagnosis and a great part of the treatment of juvenile delinquents is allotted to it, as well as the training of residential staff in schools and hostels. It is unfortunate that the reports from Aycliffe and Kingswood Classifying Schools were apparently too recent for consideration alongside Stott's work and the painstaking American study of the Gluecks on the 'After Conduct of Discharged Offenders'; they would have provided Dr Edelston with some useful statistics, although surprisingly he does not value such evidence highly.

In addition to the conventional classification of delinquencies, he lists an 'Emotionally Infantile' group. Although disturbed by it, as everyone working in the field must be, he can offer no certain basis for diagnosis or cure. For some, he is led to suggest a 'sentence' of fifteen years or so in order that they may acquire the necessary maturity not to commit delinquent acts, but it is doubtful whether either the legal profession or the Howard League would wholeheartedly approve of this.

There are few conclusions in this book with which other psychologists will disagree; to prevent hopes running too high, however, it should have featured its subtitle more closely—'A clinical Study from the Child Guidance Clinic'. JOHN M. GIBBS

Rorschach's Test: III, Advances in Interpretation. By Samuel J. Beck. New York: Grune and Stratton. 1952. Pp. viii+301. $5.50.

The third volume of Dr Beck's impressive work on the Rorschach test is the result of his continued experience in both research and clinical practice. The book falls into three sections. The first outlines the author's concept of personality and states the advances in interpretation; the second, and longest section, consists of illustrative records and 'blind' interpretations together with the available clinical notes on the subjects, while the final chapter makes some fair comments on the present standing of the test and on past and possible lines of research. While primarily of interest to those immediately and practically engaged in diagnosis and treatment, the book will interest all seriously concerned with the problems of Rorschach research, particularly problems of validation. Dr Beck is one of the great exponents of the test, and he impresses with his caution. Throughout he notes those rules of interpretation which have been statistically validated, those which he believes sufficiently supported by clinical observation, and those which are merely possible leads. It would be more convenient if the norms to which he frequently refers were published again with the text. The interpretations excite admiration, but in a busy clinic the time for similar detailed use of the test is seldom available. Perhaps the most satisfying feature of the work is the eschewing of special Rorschach terminology and the use of the more familiar language of psychology. This is not an easy book to read. It is written in a rapid, machine-gun style, with a misleading effect of dogmatism; ugly words and phrases are frequent, e.g. 'concretistic', 'synthesizing a expectancy'. Most certainly it is not matter for beginners. For the experienced Rorschach student it is in places wonderfully illuminating, at times very controversial (especially if the student has been educated along Klopferian lines). It is stimulating throughout. B. M. SPINLEY

The Perception of the Visual World. By James J. Gibson. Boston: Houghton Mifflin Co (London: George Allen and Unwin). 1950. Pp. xii+235. 35*s.* net.

Though many readers will have seen this book when it was originally published in 1950, many also will be glad that it is now issued and readily obtainable in this country.

The book is of great value in that it presents perceptual theory in a new aspect, and does much to rescue it from the slough into which it had fallen during the years succeeding the publication of Koffka's *Principles.* Gibson's main thesis is that the 'visual world'—the world of everyday experience as distinct from the field perceived momentarily by a single stationary eye—is on the whole very much alike for everyone. In particular, our apprehension of visual space, and of the qualities of shape and distance of objects which are related to visual space is normally clear, unambiguous and to some extent unlearnt

The 'visual world' is perceived by virtue of the fact that the visual stimuli we receive possess series of graded qualities of size, shape, colour, texture, and so forth; and our phenomenal impressions are accurately correlated with these gradients and with their spatial and temporal changes.

Gibson then discusses in more detail the various types of gradient which may be supposed to relate to the phenomena of depth and distance, 'constancy' and stability of objects, and so on. Here he is rather too prone to suggest that, because it seems likely that a particular gradient may be utilized, for instance, surface texture in the perception of depth and shape constancy, therefore we can assume that it is so used. His beautiful illustrations are carefully chosen to reinforce the suggestion. But he rarely presents any experimental evidence of the use of any particular gradients, except in relating perception of distance to the gradient of deformation of the image arising during motion of the observer. It is to be hoped that his suggestions may stimulate further research in which these gradients are studied rather than the elementary sensory qualities and their physical concomitants to which so much attention has been devoted. The concept of the *correlation* of phenomenal characteristics with physical qualities of the stimulus is a most valuable correction to the old concept of the transformation of the latter into the former.

Gibson's thesis is also designed to contravert the hypothesis that our percepts are individually constructed in the light of 'meanings' which we have learnt by experience to attribute to them. It may be conceded that in certain cases 'meaning' may be constituted in terms of form, colour and spatial arrangement, without reference to specific known objects. It may also be true that these 'meanings' are unlearnt, as are the significant forms which Lorenz has shown that animals respond to instinctively. Indeed it seems more than likely that the phenomenon of three-dimensionality, for instance, is immediately perceived without any need to learn by experience that it exists. On the other hand, there seems good reason to suppose that these basic unlearnt phenomena are completely incorporated into perceptual schemata which must of necessity differ from one person to another in so far as their experience differs. Gibson has done good service in outlining the stimulus qualities which appear to determine our percepts; but it may be that in studying their operation it is better to reserve judgement as to whether we learn to employ them or can do so innately. M.D.V.

Art and Everyman. By Margaret H. Bulley. London: B. T. Batsford Ltd. 1951. Vol. I. Pp. xvi+75. Vol. II. Pp. xii+91. Complete work in 2 vols. £4. 4*s*. 0*d*. net. Single vols. £2. 5*s*. 0*d*. net each.

Magnificent in the variety and range of its illustrations, this book stands as the culmination of Margaret Bulley's work during the last twenty-five years. It follows her usual plan of reproducing pairs and groups of pictures, pots, fabrics, pieces of furniture, buildings, etc., which are similar enough for ready comparison but different in aesthetic merit. In vol. I the reproductions are arranged in groups to illustrate aspects of design (mass, line and space, colour, light and shade) and the text for each section gives a general introduction to the topic followed by commentary on each plate, on the plan of an illustrated lecture for a popular audience. The intention is didactic; Miss Bulley systematically inculcates her own taste, and would emphatically deny that she does. She believes firmly in a standard of aesthetic judgement independent of individual preferences but offers no evidence for it that I find convincing. Instead she reiterates in slightly varying form the suggestion that the 'seeing eye' recognizes in a work of art 'the true mind-picture' and 'the harmonious effect which is its sign'. The basic approach to the audience is always, in effect, 'You can see that this is better than that, can't you?', and usually we think we can, especially because Miss Bulley skilfully draws our attention to the features of the design that count for most with increasing familiarity and continued attention. To psychologists one great value of her book lies in its excellent collection of material for the study of aesthetic preference. Another consists in its implicit challenge to those of us who differ from her aesthetic theory to define a better one ourselves and to be sure that it takes account of preferences no less subtle and disciplined than Miss Bulley's.

The second volume includes a section in which Miss Bulley herself discusses the inadequacy of the Freudian and Jungian systems to contribute to an understanding of what she means by art. Although her explicit criticisms could no doubt be answered, the fumbling debility of these (and other) psychological systems in face of the problems of discrimination with which she comes to grips reveals itself clearly enough. Few psychologists would agree with her central contention that there is a clear demarcation between 'art' and 'not art' (which in earlier books she has called 'counterfeit') and no intermediate group of work partaking in some degree of both; but let them, as she suggests, demonstrate their point of view as effectively as she has demonstrated hers. The second volume of the work deals with more

abstract conceptions such as these, but still by means of the detailed comparison of actual works (of which there are over 800 photographs). The great achievement of the two volumes is the immense range of illustrations, organized coherently by a consistent point of view and a long-practised attentive judgement. D.W.H.

Factor Analysis. By R. B. Cattell. New York: Harpers. 1952. Pp. xiii + 462. 48*s.* net.

Few psychologists can have carried out as many large-scale factorial analyses as Prof. Cattell, and he must certainly hold the record for rotating to oblique Simple Structure. Thus a book which sets out the fruits of his practical experience, and his broader speculations on the nature and use of factorization, could not fail to be of value. It is indeed a fine piece of work—clearly and very objectively written, and packed with interesting ideas and helpful illustrations. Parts of it, however, are very heavy-going and the book hardly succeeds in providing the working manual which any Ph.D. student could follow, for which there is a great need. The first Part does present a fairly simple survey of the whole topic of centroid analysis, which should be useful to many Honours students; nevertheless, the reviewer would still be inclined to recommend Sir Godfrey Thomson's book in preference. It is the more complex second Part (dealing with working methods and particularly with the art of rotation), and the third part (on general principles and problems), which add most to the standard text-books by Burt, Thomson, Thurstone and Holzinger.

Factor analysis, for Prof. Cattell, is by no means confined to exploratory studies of mental organization and classification. He makes ambitious claims, and presents strong arguments, for its wider use in general psychological research, and its combination with classical experimental, or with modern Fisherian, techniques. The psychologist or statistician who is suspicious of the whole topic should certainly study Parts I and III with care. A number of doubts, nevertheless, still remain in the reviewer's mind. Apparently any really adequate analysis involves giving some sixty tests to a few hundred subjects, extracting a dozen factors, and spending about six months full-time on computation and rotation (even with machine aids). But scarcely one in a hundred professional psychologists could undertake research on such a scale; indeed scarcely one in a hundred is likely to be able to learn how to carry out blind rotation to oblique Simple Structure. Either then we must be allowed to carry on with simpler researches involving less than twenty variables and half a dozen factors, with orthogonal rotation or no rotation at all, or else factor analysis looks like becoming an even more esoteric and specialized approach, even more divorced from the main stream of psychology, than at present. Finally, if factorization is so fundamental to any advance in psychology, why is there still so little agreement regarding the basic components of intellect and personality which it is supposed to reveal? These are some of the questions which Prof. Cattell has not answered; though it is a little unfair to raise them in view of the vast number of points—not mentioned in this review—which he does cover. P.E.V.

The Journal of Applied Statistics. Ed. by L. H. C. Tippett. Vol. I, no. 1, March 1952. Edinburgh: Oliver and Boyd Ltd., on behalf of the Royal Statistical Society. Pp. 80. 10*s.*

Statistical methods are still developing rapidly and finding an increasing number of users among administrators and executive officers, as well as scientific research workers in the whole range from sub-atomic physics to the social sciences. A specialist in one field may derive more helpful suggestions from descriptions of ways in which statistical methods have been successfully adapted for dealing with problems arising in other fields than from formal expositions in general, often highly mathematical terms—as psychologists often find when they consider the analogies between educational courses and fertilizers, examination results and body-weights of fat stock, etc. By bringing together articles concerned with the widest possible range of applications, the *Journal* should facilitate such an interchange of ideas and may well prove helpful to a large number of readers, to anyone in fact who regularly applies or consults statistics.

The article on 'Statistical Methods in Industry' by B. P. Dudding, of the General Electric Co., in the first number, gives a very enlightening account of how co-operation can be maintained between the statistical research service and the general management and production departments in a large industrial undertaking. There is also an interesting article by L. T. Wilkins, of the Social Survey, on estimating the social class of towns, with comparative figures for most of the larger towns in England; and several

examples of the application of sampling methods to problems of inspection. Psychology is represented by an article by H. J. Eysenck on 'Uses and Abuses of Factor Analysis'.

The *Journal* also reports the activities of the Industrial Applications Section and the Study Section of the Royal Statistical Society. It will be issued three times a year; single numbers are 10*s*. and the annual subscription is 25*s*. (U.S.A. $4.00) post free. The publishers are Oliver and Boyd Ltd., Edinburgh.

P. SLATER

Contributions to the Theory of Accident Proneness I and II. By GRACE E. BATES and JERZY NEYMAN. University of California Publications in Statistics, Vol I, nos. 9 and 10. University of California Press. 1952. Pp. iii + 139; ii + 21. $0.50 and $0.35.

In the first of these papers the authors consider the problem of selecting people for occupations so dangerous that any accident is likely to be fatal. The concept of accident proneness due to Greenwood, Yule and Newbold, which implies that individuals differ in their liability to a particular type of accident and thus that those who have had accidents in the past will be more likely to have others of the same type in the future, is not of much use here. Bates and Neyman therefore generalize this concept by assuming that an individual whose proneness to one sort of accident may be measured by A, will have a proneness to some other sort measured by kA, where k is the same for everyone. Using this assumption, together with the usual ones of a Poisson distribution of accidents for each person separately, and a Pearson Type III distribution of accident proneness in the population, they obtain the multivariate negative binomial as the theoretical joint distribution of numbers of different sorts of accidents. This distribution appears to fit some new data of industrial accidents remarkably well, but is not so suitable for the accident data of London bus drivers collected by Farmer and Chambers.

In the second paper an alternative model is discussed. It is already known that the same negative binomial distribution may be obtained either from an assumption of accident proneness or from an assumption of contagion, where by this is meant that anyone who has an accident becomes thereby more likely to have another. It is clearly important for practical reasons to distinguish between these two cases and the authors show that this may be done by considering the joint distribution of numbers of accidents in successive periods of time. The actual tests to be performed, and their efficiency, they expect to discuss in a later paper.

The problems discussed should be of great interest to psychologists, and the notion of contagion in particular is most important. The theoretical exposition, however, would be extremely difficult to follow for any one not well acquainted with probability generating functions. The first paper gives some interesting data of the joint distribution of numbers of certain industrial accidents. V.R.C.

The Prediction of Academic Success. By H. H. HOHNE. Melbourne: Australian Council for Educational Research. 1951. Pp. vi + 159 and viii + 70 of tables and graphs.

Intelligence and High Level Achievement. By C. J. ADCOCK. Wellington, N.Z.: Victoria University College. 1952. Pp. 26.

These two mimeographed reports represent different but complementary approaches to the complex problems of academic performance at university level.

Dr Hohne's is the longer and more substantial paper. It runs to some 160 pages of text, and an accompanying volume of 70 pages of tables and figures contains a tremendous amount of information about the subjects of his investigation. Begun as a research into the predictive value of psychometric tests, it became very largely an inquiry into the problems of student wastage at Melbourne University. The first two sections contain a very detailed analysis of the backgrounds, school records, matriculation results and performance on psychological tests of freshmen entering the university in 1943 and 1944 and of their progress through their university courses.

This report is directed to university teaching and administrative staffs rather than to research workers in selection methods. For this reason, among others, conclusions from the investigation are for the most part presented in terms of students' chances of graduating in minimum time, graduating after changing courses, obtaining honours and so on, rather than by means of correlations. The third section, which is concerned with prediction, contains a chapter on the value and limitations of correlation techniques

in such an investigation, and another on the predictive value of certain psychometric tests. An interesting chapter examines the claims made for the group Rorschach test scored and evaluated in various ways. Dr Hohne finds that a somewhat subjective 'clinical' method of interpreting the Rorschach protocols alone provides additional worthwhile information for purposes of prediction. A chapter on prediction from combinations of tests includes a discussion of a method of 'pattern tabulation' of test scores derived from Cronbach.

Dr Hohne believes that there is a good case for the inclusion of psychometric tests in any scheme of selection but he is fully aware of the great importance of many factors which cannot be touched by any tests. The case for a proper service of psychological advice and guidance receives powerful support from the evidence he adduces.

The population studied in this investigation is not typical of a British university. It consisted largely of wartime students entering under special arrangements, of part-time students and adult matriculants, and the wastage rate by our standards was enormous. Some of the detailed information is therefore likely to be of mainly local interest, but the thorough and penetrating discussion of the problems inherent in the selection of university students and some of the techniques employed deserve careful study. It must be noted that the present report is concerned only with students from the Arts Faculty. Problems are likely to be rather different with science, engineering and medical students. Results from other faculties are promised. This first report is a very solid and worthwhile piece of work.

Dr Adcock's short paper (26 pages), which is No. 1 of the Victoria University College Publications in Psychology, is both more theoretical and more technical than the Australian report. A large battery of cognitive tests and ratings on a number of temperament traits was used in a pilot investigation with a group of fifty students in 1948 and a shorter series in the main research the following year with three groups taking different courses and at different stages of their university careers. The results were submitted to factorial analysis. A number of problems in technique and method are considered, and there is some discussion about the significance of the factors extracted. The correlations between test scores and examination marks and the factors involved in university success are also briefly discussed. Dr Adcock emphasizes, what is apparent from the results, that there is remarkable variation in the factor pattern which emerges with different groups, and stresses the importance of non-intellectual factors in the prediction of success. In these, as in several other points, the two reports reinforce each other's conclusion. It is clear, as both authors conclude, that there is room for a lot more research on these problems. A.H.D.T

Human Nature, its Variations, Development and Assessment. By JOHN C. RAVEN. London: H. K. Lewis and Co. Ltd. 1952. Pp. xii+226. 12*s.* 6*d.*

Mr Raven has written an introductory book designed to give a 'concise psychology' for those concerned with the welfare of others. It has the width of interest and understanding that one would expect together with flashes of wit and insight that delight the reader. The style is pleasant and uncomplicated.

The book is in three parts: the first deals with development from birth to maturity, the second with variations in experience and their explanations, and the last with assessing abilities and interests by test procedures. There is a preamble on organic development and an appendix on statistical method.

It is no easy task to write an elementary book on psychology, and Mr Raven has provided one, giving allegiance to no particular school of thought, that has an individual quality lacking in such books. The subtitles are provocative and interesting, although at times the link of thought between the sections is not entirely clear. The first two parts of the book will probably prove of the greatest value to the ordinary reader. The section on mental testing is honest, and shows both the uses and limitations of the available test procedures. Some of it is perhaps too technical for the ordinary worker in social spheres. A.M.

Reporting Research in Education. By STEPHEN WISEMAN. Publications of The University of Manchester School of Education, no. 2. Manchester University Press. 1952. Pp. viii+24. 3*s.* 6*d.* net.

This pamphlet offers concise recommendations to research students on the writing and layout of theses on 'experimental education' (which seems very much like educational psychology). Besides dealing with the more mechanical features of the job, such as the use of appendices and the preparation of a li

of references, the author gives valuable suggestions on the planning and writing of a thesis. There is also a short and sensible section on preparing a paper for publication. The pamphlet should be of the greatest value to research students, its succinctness setting them an excellent example to reinforce the author's remark that 'it will be rare indeed to find a "good" thesis an inch or two inches in thickness'.

King Solomon's Ring. By KONRAD Z. LORENZ. London: Methuen and Co. Ltd. 1952. Pp. xix + 202. 15*s*.

Lorenz has written a charming and unusual book which will be read with pleasure by biologists and laymen. The freshness and simplicity of the style suggest that the translator did an excellent job. The author's marginal illustrations are vivid and amusing. Because it is anecdotal, discursive and personal the reader may find himself disagreeing as often as he agrees, but this adds to the interest of the book. Although apparently written to please rather than to instruct, some of the information about animal behaviour is an original contribution of importance. The long account of the author's free-living jackdaw colony would demand the attention of the social and comparative psychologist even if it stood alone; while the niceties of the water shrews' way of life and unusual method of orientation within its environment are of considerable scientific interest. *King Solomon's Ring* is full of challenging ideas and possibilities, every chapter suggesting problems ripe for research rather than cohesive and polished theory. It comes as a pleasant surprise that a naturalist as distinguished as Lorenz, who has made such an outstanding and widely valued contribution to the scientific study and understanding of animal behaviour, should have so little of the morbid fear of anthropomorphism which sometimes renders studies in comparative psychology sterile and pointless. It might be inferred from this book that the biologist who goes down on his hands and knees to talk in quacks with a mallard duckling on its own level, may be as blessed in scientific reward as his more cautious fellow who sticks to the rat, the oneway vision screen and the conventional apparatus of science. M.M.L.

Mind and Life. By SIR ARTHUR TANSLEY. London: Allen and Unwin. 1952. Pp. xii + 171. 15*s*. net.

This all too short book consists of a series of essays ranging over a wide field of topics, directed to the reconciliation of a loyally accepted scientific basis with the objective validity of values. The author's solution appears to depend on a doctrine of emergence. The human organism is fundamentally and strictly a physical system, but those of its functions called mental become a system which can be regarded as an independent entity. Many readers will feel that the writer started with deep convictions, and then set out to find arguments to support them. In the end he has to say that 'we can only have faith in spiritual reality as the result of a deep conviction within our own minds that we are spiritual beings as well as animals'. The defect of the book is that, with its extensive range of subjects, the arguments are at no point driven home decisively. Sir Arthur modestly disclaims originality, and in fact his book will be more profitable as a stimulus to the general reader, than to those psychologists who have already given thought to these matters.

As we should expect, it is charmingly written, and the sensible discussions and criticism of McDougall's theories, and of psycho-analysis, may be singled out for high commendation. A.W.W.

If a Man be Mad. By HAROLD MAINE. London: Gollancz. 1952. Pp. xii + 435. 15*s*. net.

This is one of several recent books describing mental disorder, and the treatment available for it, from the patient's point of view. The author in this case is a professional writer whose chief symptom was alcoholism. His experience in several types of American mental hospitals and his recovery in spite of the treatment he received are described vividly for the general reader. It represents part of the popular outcry which may lead to improved conditions for the insane and create a better understanding of insanity among the general public.